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MR. ABERNETHY'S

PHYSIOLOGICAL, PATHOLOGICAL, AND SURGICAL OBSERVATIONS;

DELIVERED IN THE

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At St. Bartholomew's Hospital.

The Muscles on the Front of the Body.

I SHALL proceed to give an account of the muscles that are found on the front of the body; and first, of those that are found on the front of the neck. It is a rule, in dissecting, to detach the muscles from those attachments which are most plain and obvious, and to leave them attached to those parts which are the most obscure; because you can afterwards examine those parts deliberately.

Here I find the *subcutaneous* muscle. Animals have those muscles generally, by which they wrinkle the skin and strike off any thing that may be annoying them—frighten away any fly that may be disposed to bite them. But as man can apply the hand to every part of the body, there was no need of this subcutaneous muscle; yet there is here a fleshy thing that bears the name which Galen gave it, *platysma myoides*. This muscle is stronger in front than in the back part: much need not be said about it. As to its *action*, it tugs up the integuments beneath the collar bone, and tugs down the integuments of the face.

Then there are certain practical observations respecting muscles; which I deem worthy of attention; and one is, that the *external jugular veins* run down beneath this muscle; and if you open an external jugular vein, you had better carry your knife in the direction of the fibres, and not go against them, otherwise unpleasant circumstances may be produced. Also I know, that where there are *cysts* in the neck, in the situation in which I have often seen them, that the tension of the fibres gives a firmness to them, which would make you think there is something solid where there is actually nothing but fluid. However, this muscle is raised, and then you see beneath it another, which is a very remarkable and a very important muscle, a muscle which comes from the *mastoid process*; its tendon is partly attached to the process, and partly spread over it, and it is named, from its attachment, *sterno-cleido-mastoideus*.

Well, what will that muscle do? Of course, bringing the mastoid processes towards the *sternum* and collar bones, the head is brought forwards. But suppose one of the muscles will act and the other will not; suppose the right one does act and the left one does not; why then that will bring the right mastoid process down, and twist the neck. Again, the muscle may act in a sort of irregular manner, and, without approximating the attachments, it may bring them opposite to one another; for instance, if I turn my head to one side, I do it with this muscle, yet I don't make the muscles approximate. Now this case, with respect to this muscle, seems to be well known to every artist—to every gentleman who has a taste for an artist. In making a figure of the head turned round, what do you see? Why, every gentleman understands that it's the sterno-cleido-mastoideus which presents itself to view, and it would be shameful for a medical man to be ignorant of it.

Now for practical remarks:—There's a *wry neck*. Cases of wry neck are no uncommon occurrences, and they seem to indicate undue actions of the sterno cleido-mastoideus. I have always said, in these lectures, that a very pretty little book might be written on wry necks; for sometimes inflammation in the glands, in the neighbourhood of this muscle, may excite it to this irregular action. Something on the opposite side of the neck, preventing muscular action, may cause the neck to be twisted; but the muscles on the side to which the neck is twisted are not the faulty muscles.

Then, as to wry necks in general, they are the result of the irregular action of muscles; and many muscles are concerned in the production of the wry neck. Now in those

cases of wry neck which result from the irregular action of muscles, if the sterno-cleido-mastoideus be chiefly affected, what is to be done? Why, you must endeavour to tranquillise the muscles. There are many of those cases entirely the result of a disturbed state of nerves, as I believe, caused by disordered digestive organs: and whether this be universally the case or not, I will tell you of one absolute, undoubted instance of it. This happened in a tall boy at school; he was seized with a wry neck, they leeched him, and blistered him, and made bad worse, and after a week he was sent up to town. I say he was a tall, lanky boy; and, upon my life, I thought a *pullet's* neck could not have been more twisted round than this was: that, you know, admits of a particular turn from the mechanism of the vertebræ of the neck, in order to let the fowl put its head under its wing to go to sleep. But so much was this boy's neck twisted, that I told him to lie on a sofa, or couch, or bed, and to support the head with pillows; never to sit upright; never to put the head in a position to demand muscular action for support, since, if the muscles did act, they would act in this faulty manner. I told him to foment his neck by flannels; to keep it in a kind of tepid bath; to keep himself in a kind of perspiration; and I applied the whole of my attention to put his digestive organs to rights. His bowels were all wrong—his tongue furred, and of a bad colour. A week elapsed, and his stomach and bowels got into a decent state, and his wry neck was entirely gone. But you see if this is neglected in the outset, then muscles get a habit of perverse action, and you have wry necks established beyond removal, even by putting the digestives to rights, though that I believe to be a most direct and efficient mode of cure in every instance. But I know that there are cases of wry neck, where people have their necks twisted, and the muscles put into such an irritable state of action, that the cases do not yield quickly, even to what I should consider the most judicious and appropriate treatment. But, now, in reading books of surgery, you find the proposition of dividing the sterno-cleido-mastoideus muscle, in order to set people's heads right on their shoulders. Now, I did live for a very great part of my life, and I did not know what the plague those people meant by this; I never could meet with a case where such an operation could for a moment have been rationally thought of; then, as curious cases are something like misfortunes, that is, they never come alone, in one year I met with *four* cases requiring an operation in the sterno-cleido-mastoideus. All these cases appeared to me to be of the same nature; I mean to say, it appeared to me that the muscle appeared to be originally *mal*-formed; that the clavicular part of the muscle was shorter than it should have been; it was irritable, and drew the mastoid process towards the collar bone. And *note* the consequence: the people—children they were, for they were not more than fifteen or sixteen years of age,—had all grown wry in their backs; the vertebral column had become completely deformed. And it is natural to suppose that that would be the case, for if any thing oblige a man's head to lie pretty much in contact with his shoulder, he is under the necessity of bending the vertebræ of the loins to the opposite side in order to support the gravity of his head perpendicularly upon the *sacrum*. The inversion of one vertebra, creates the necessity for another inversion. *Hang* it, says I, these children shall not remain in this manner; so I made a longitudinal incision in the sternal portion of the muscle, cut it fairly through, and up started the head perpendicularly upon the neck. I did not touch the clavicular portion of the muscle at all; that did not seem to be at all faulty, and when the clavicular portion was set right, all seemed right; and I have great satisfaction in informing you, that these children became straight in their backs. Now that 's a very curious thing, that the curvatures of their spines became involved without any mechanism. In one of the children, indeed, a second contraction took place after about a year and a half. I shall not stop now to explain the process, that 's explained in the surgical lectures, but I divided it a second time, resolved that the child should grow up to its full stature without deformity of the vertebral column. I have seen several cases since; and there was a student here who chose to have the operation done on his neck; not that it was in any considerable degree deformed. He would have done very well for a *courtier* in the court of Alexander the Great, where it was the fashion, in imitation of their monarch, to walk with their heads a little twisted. (Laughter.) But, however, he chose to have it done, and done it was, and with a very satisfactory result to him; he then could do with his head what he could not have done before.

With respect to the *depressors* of the *larynx*, there are six of those muscles; but I have not stopped to mention their actions separately, for it would have been needless; they all have the same actions, namely, to draw down the larynx towards the sternum.

With respect to the *elevators* of the larynx, the first I have to point out, is a muscle which is generally known by the name of *digastricus*. As to its *action*, why, I say, we never can tell what the action of a muscle will be; it depends upon what are the fixed parts of the muscle. Admitting, then, the

jaw to be the fixed part of this muscle, it will raise up the *os hyoides*; it is an elevator, then, of the os hyoides. This muscle has been somewhat dignified by a dispute between two great teachers of anatomy, namely, Dr. Hunter and Dr. Munro; not the present, but the past Dr. Munro. Now those two great men never would have disputed about this, if they had made up their minds to have done what I tell you, you must unquestionably do, which is, first of all to determine which is the fixed part of the muscle. The dispute was, whether the *anterior belly* of the digastricus would not depress the jaw? Unquestionably, if the os hyoides is the fixed part, then the anterior belly of the digastricus would depress the jaw. The jaw wants no depressor in general; it will fall by its own weight, as you will see in a man going to sleep. But suppose a surgeon was operating on a child, and he were to say, "Don't cry, you little rogue," but the child bellowed lustily, what would he do? He would draw the jaw down as far as possible, and in doing that, there is no doubt that he would use the anterior belly of the digastricus to effect this purpose.

The *stylo-hyoideus*: They tell a trivial circumstance about this muscle, which is, that some of the *tendons* spread in going over the digastricus muscle, and others pass behind. It is a poor trumpery muscle, and it's not worth while to be talking about those minute parts of anatomy that belong to it. It must be considered as an elevator of the os hyoides.

Then there are two more muscles which elevate the larynx, the *mylo-hyoideus*, and the *genio-hyoideus*. There are, therefore, three muscles on each side to depress the larynx, and four to elevate it.

Now I remember, when I was young, people used to say, "O, it's not necessary to study anatomy, taking into consideration all these small muscles; *damn 'em*, they would puzzle any body." They used to call them the *little* muscles with the *long names*. Now I was pleased to hear that, for it always gave me an opportunity of saying, it would be a great desideratum to have all the muscles named the same, in one way. I heartily wish that that most laborious man, now dead, he who had such great knowledge of the languages, *Dr. Barclay*, had but made the attempt. What's the use of calling a muscle *oblique* or *straight*? Does that tell you where it is, what it does, or any thing about it? There are many muscles named after mathematical figures, to which they bear no similitude whatever; but I defy any body to hear the name of sterno-cleido-mastoideus pronounced, without imagining the muscle; it recognises the attachment of the muscle, and if you know the attachment, you know the use; and the use is, to bring that which is most moveable to that which is most fixed. What is the use, for instance, of calling one of these muscles digastricus? Really the whole nomenclature of muscles is quite ridiculous. But you must know the muscles if you would be anatomists, and you must be anatomists if you would be surgeons. I dare say Mr. Stanley has told you, that the knowledge of the bones is the foundation of anatomy. Aye, I dare say he has; and now I have to tell you, that the knowledge of the muscles is the next in superstructure. How is it possible you can know the arteries and veins, unless you know the muscles? Come, now, I would ask you, What is the situation of the *sub-maxillary salivary gland*? You must answer me, that it lies immediately below the jaw, and upon the mylo-hyoideus muscle. Now I ask, What is the situation of the *sublingual gland*? It must be answered, immediately under the tongue, and upon the mylo hyoideus. The oblique direction of this muscle is the cause of the unequal depths in the mouth; the mouth is very shallow in the front, but it deepens behind; and what is that thing you touch with your tongue. It is the mylo-hyoideus muscle.

But I cannot quit the subject yet, for I know it must be said, it must be very strange that the larynx, which you call the organ of the voice in the windpipe, should have such multitudinous powers to raise it up. Does it facilitate respiration? Does it affect the tones of the voice? No, I don't think it does. It affects the note of the voice, as you will afterwards learn. Unquestionably, the note depends upon the situation of the larynx, but the tone is a totally different thing. But was it for this, that we might vary the note of our voices? O, no. Then why had we those powers? Because the bag which receives the food when we swallow, the bag of the pharynx, is attached to the back part of the larynx; so that the larynx is lifted up, that the bag should be lifted up to receive the food. Put your hand upon your larynx, and then attempt to swallow. *Gulp*—up and down it goes with the convulsive motions. Why does it go up? O, for more reasons than I can tell you at present; but we shall at present say, that the bag may be raised to receive the food. Why does it go down? O, that is to be answered in this way, because it is necessary for respiring. We are breathing at all times, and swallowing occasionally, and it is necessary for respiration that the bag of the pharynx should be brought down. Therefore have you these muscles; and now I have done with them: but I must show you another muscle.

Pectoral muscle. What will this do? Why, if the arm is lifted up, unquestionably it will depress the arm on the side. When the arm is depressed on the side, it will bring the arm forward; and the ridge being in front, or external to the bicipital groove, it will roll the arm in. All this is said upon the presumption of the trunk being the fixed part, but the arm may be the fixed part. Have you never seen a little boy climb up a tree? If you have, you will have observed that he catches hold of a bough with his arm, and draws up his body, which is done with this muscle; sometimes, also, he lays hold of a branch with his legs.

This muscle is well known to every artist, and gentlemen having a taste in that way, and it would be most unpardonable for a medical man not thoroughly to understand the pectoral muscle. And to show you of how much consequence it is to know the attachment of muscles, I mention this: Suppose a muscle to be burst, and subsequently inflamed, what are you to do? What! to approximate the different attachments as much as you can, and to let the fibres be as loose as possible, for then the inflammation will go down, and the parts will unite; but if you let the attachments be separated, and the fibres stretched, God knows when the inflammation will go down, or the parts of the muscle unite. But suppose the muscle to be divided; if you cut it, it gapes, it retracts by the living power, and you may bring the parts of the skin together by adhesive plasters, but how do you close the wound in the muscle? There is no other way but by compression; and if you approximate the fibres, then they will unite together, and the wound will be closed. There is no other way of closing muscular wounds. You may think as you please, but I am sure that no one is fit for his profession who does not understand the attachment of the muscles, neither can he learn the rest of anatomy, without understanding the muscles: don't, therefore, suffer your minds to repose, or be contented, without acquiring this knowledge. They say when you take off a diseased breast, you should put the pectoral muscle on the stretch: I remember the time when they used to sit a patient down in a chair, when that operation was going to be performed, and put a plane under the arm to keep it stretched. They don't do the operation so now; but, of course, they put the pectoral muscle on the stretch still.

Deltoid muscle. If there is any part that surgeons should be acquainted with, it is the *gap* between the deltoid, and the clavicular portion of the pectoral muscles. I put my finger into that gap in my own person, and I stop the beating of the pulse at the wrist. Suppose a man has got a bleeding in his arm, which there is a difficulty in stopping, how are you, as a surgeon, to stop it? By passing your finger over the first and second ribs, pressing the artery, and you stop it at once. When I talk about this sort of thing, and hear a medical man say he can't do it, I always say, there are others that can. And I tell this story of a lady who was examined by a great number of *learned doctors;* they could not tell what was the matter with her; they had *never* met with such an *intermittent* pulse in the whole course of their lives, nor any thing at all to equal it, and they pronounced their opinion that she could not live a moment, and left the room; and, *egad,* all this time she was *twaddling* with this artery at the shoulder. (Laughter.)

Now I will only just say, with respect to *system,* that there is a great deal in that. In acquiring a knowledge of any science, system is almost indispensably necessary; but it is especially so in the medical profession. With this view, the abdomen is divided into *regions;* and from want of attending to this, I have known something of this nature occur elsewhere: the examiner has asked a young gentleman who presented himself for examination, "What do you see upon opening the abdomen?" The chap has stared, looked aghast, and stuttered out, "wh—wh—wh; what—what do I see, Sir?" "Aye?" "Why, th—there's, there's the *liver,* Sir." "Well?" "An—an—and there's the *stomach,* Sir. An—an—an—and—and there's, there's the *spleen,* Sir. And there's the *cæcum,* Sir." (Roars of laughter.) Now, then, just let me tell you, that you will do well to proceed *systematically,* and to *pack* up your knowledge as you go along, in such order, that, when you are asked for it, you will at once know where to find it.

LECTURES

ON THE

Diseases of the Nervous System.

BY

DR. CLUTTERBUCK.

Lecture VI.

Of Phrenitis.

Gentlemen,

Having treated of *membranous* inflammation of the brain, we come now to inflammations of the organ itself; and first those of the *acute* kind, or such as are attended with *pyrexia,* or a febrile state of system. Now the disease may be general or partial. One

might even conceive inflammation to occupy the whole organ at once, although the division and separation of parts that are found here, make it improbable that such should often, if ever, be the case: nor has the actual existence of any such case, I believe, been proved by dissection. Indeed it is difficult to imagine the entire mass of brain to be at once the subject of inflammation, consistently with life. It is very probable, however, that one hemisphere of the brain may be inflamed, without the opposite one, just as happens with regard to the lungs. We have, indeed, a tolerable proof of this in cases of hemiplegia, where the disease is confined to one side of the brain, as shown both by the symptoms and by the appearances after death. It is reasonable also to believe, that the different lobes of which the hemispheres are composed, may be separately affected, and give rise to peculiar symptoms, as they have no doubt different functions to perform. It is certain, likewise, that the *cerebellum* is very distinct in its uses from the *cerebrum*; and it is hardly to be questioned that the different structures found at the basis, under the denomination of *corpora quadrigemina*, *pons Varolii*, *medulla oblongata*, &c. (made up of these parts are of processes from the *cerebrum* and *cerebellum*) have each their peculiar offices, and give rise to peculiar symptoms, when diseased. Anatomy as well as observation have gone some way toward proving these positions. But our knowledge of the subject is still far from precise or satisfactory. Experiment itself is extremely fallacious in matters of this kind, and has furnished results in many respects contradictory. Much yet remains to be done upon this interesting subject, before the pathology of the brain can make any considerable advances. Happily, however, such minute distinctions as I have just alluded to, are by no means essential to successful practice; for we act upon general indications, which are not much influenced by the particular seat of the disease. It is enough, in general, that we can assign the nature of the disease present, especially as founded in inflammation or otherwise; a point that may in most cases be determined without much difficulty, and with sufficient certainty for practice. And in all doubtful cases, you should make it a rule to act as if inflammation were actually present; but, of course, with the greater caution, in proportion as the case is uncertain.

Now without attempting to make a division of the different inflammations of the brain upon strict anatomical principles, (for which, as I have shown you, there is as yet no proper foundation), I shall confine myself to the varieties that authors have principally noticed, and which are founded chiefly upon the apparent disorder of functions. Those of the *acute* or febrile kind may all be included under the terms *phrenitis* and *idiopathic fever*; for I shall assume for the present, that the latter, as well as the former, is merely a variety of inflammation of the brain; leaving the proofs of this opinion, to a future occasion.

1. *Of Phrenitis.*

This term has been applied by authors to that variety of cerebral inflammation, which is characterised by great and active disorder of the mental functions. Accordingly, Dr. Cullen, following the generality of authors, gives us, as a part of the definition, "furious delirium;" as if this were a necessary symptom of inflamed brain. He mentions, however, as an alternative, *typho-mania*; by which is to be understood, *delirium with a degree of stupor*. But neither one nor the other of these is necessary to constitute the disease; as you will presently see when the symptoms have been fully described. This form of inflammation of the brain, has been sometimes called *phrenismus*, *cephalitis*, *sphacelismus*, &c.

Symptoms of Phrenitis. In pointing out the characters of this disease, you must call to mind the distinction I formerly made of *symptoms* in general, into *common* and *proper*. Now the *common* signs or symptoms, viz. *heat*, *redness*, *pain*, and *swelling*, by which we ascertain the presence of inflammation, will not always avail us here, as you will presently perceive when we have examined those signs individually. Nor is *pyrexia* more to be relied upon. We are obliged to resort, therefore, to the *proper* or *special* symptoms, in order to form our *diagnosis*, and which consist chiefly in a disturbed state of the *sensorial functions*.

Now as to the *common* symptoms of inflammation:—

Heat of head is a frequent sign of inflammation going on in the brain, and which is observable in all the varieties of the disease, and when present, it may be much relied upon. But slight affections of the sort may be unattended with this effect, at least, in any perceptible degree. *Redness* can only be inferred to exist, from its extending, as it occasionly does, to the eyes and face. This redness or suffusion of the face and eyes, though only occasional, makes, however, a part of Dr. Cullen's definition of *phrenitis*. *Pain in the head* is another frequent attendant on *phrenitis*, especially at the outset of the disease, that is, while feeling and consciousness remain. At a later period of the disease, it is seldom complained of. The pain is often of a throbbing kind. The degree and danger of the disease, however,

bear but very little proportion to the pain the patient experiences; which, on the contrary, is often the greatest where the danger is the least; and *vice versa*. There are, you will recollect, two causes of this anomaly; one is, the natural insensibility of the brain; the other, the loss of consciousness which is so common an attendant of brain-affections. As to *swelling*, this, it is plain, cannot take place with regard to the entire organ, except in the individual case of infants, where the bones are not yet firmly united; in these, a protuberance at the fontanel is very observable, when inflammation is going on in the interior of the skull. *Pyrexia*, again, which is one of the *common* signs of inflammation, wherever seated, is not always present in this, more than in others. Indeed, in some of the worst cases, where the brain is in a state of oppression with regard to its functions, the general vascular action of the system appears to be kept down by this very circumstance, the pulse deviating but little from the natural state. On many occasions, the general circulation is greatly though variously disturbed: the pulse being sometimes extremely hurried, sometimes preternaturally slow; and not unfrequently irregular also.

We must in general, therefore, look beyond the *common* signs of inflammation, in order to discover the existence of *phrenitis*, and inquire into the state of the *sensorial functions*, where the unequivocal signs of the disease will be found. Now the manner in which those functions are disturbed, will of course depend upon the degree of the disease, and the particular seat it occupies in the brain; and will therefore be extremely various.

1. *Sensation.* This function of the brain is very generally, though not always, disturbed in *phrenitis*; and the disturbance shows itself in different ways, and in the different organs of sense. Thus the *eye* is often too acutely sensible of light, and the *ear* of sound. Sometimes those senses are impaired, or wholly lost. And sometimes false perceptions arise, not produced by any external cause, but by a disordered condition of the origin of those senses in the brain. Thus it is that imaginary objects are seen; flashes of light are complained of; or every thing around the patient appears to him to be on fire; the colours of objects are changed to his imagination: and the same may be the case with regard to the organ of *hearing*. The other senses of *taste*, *smell*, and *touch*, are liable to be equally deranged: thus there are fancied *tastes* and *odours*: while a sense of *creeping* on the skin, or a feeling of *cold*, or *heat*; or an extreme degree of sensibility to the *touch*, and often actual and acute *pain*; are among the occasional symptoms of this affection.

2. The *voluntary power* is not always affected in *phrenitis*; the disease, in its strictest and simplest form, being an affection of the *mental function* only. It is seldom however so simple as this: not only *sensation* being disturbed, as I have just described, but the *voluntary power* also, and that very variously. In some cases it is increased in energy, enabling the patient to make the most extraordinary muscular efforts; in others, the reverse state, *prostration of strength*, occurs; and this is generally the case towards the end. At other times, there are feeble and tremulous movements; and, occasionally, epilepsy or general convulsions take place.

3. *The mental functions.* Inflammation may, and very often does, exist in the brain without any disturbance of the mental powers: but in the variety of the disease we are now considering, such disturbance is the characteristic mark of the disease. The *mental* disorder shows itself in various ways. It often commences with an unusual flow of spirits, with great loquacity; which is succeeded by confusion of thought, incoherent speech, and irrational conduct, up to the pitch of fury at times; and generally, then, is accompanied with high febrile action, so as to agree pretty well with the definition given by writers. Or, the ideas may be of a gloomy and melancholy cast, leading frequently to despondency; or there may be stupor, or abolition of the mental faculties, whence the term *typho-mania*, which Dr. Cullen introduces into his definition of *phrenitis*. These varieties in the state of the *mind* and *feelings* of the patient, indicate nothing certain with respect to the corporeal state of the brain, and afford hardly any guide in regard to practice; as I shall presently have occasion to observe to you.

In addition to the symptoms now mentioned, there are often others that assist us in forming the *diagnosis*: such as heat of head, throbbing of arteries, redness of the eyes, and a peculiar and generally quick expression of countenance, depending partly upon a contracted state of the pupils, and partly upon a sharpness of features, the effect of muscular contraction. The *sleep*, likewise, is hardly ever natural; in most cases there is extreme watchfulness, and that continued often for many days.

You will find it stated by authors, that *true* or *idiopathic phrenitis*, (*phrenitis vera*,) is a very rare disease, while the *symptomatic* is exceedingly common. Now you might imagine from this, that two different diseases were here spoken of; and that the latter, in fact, was hardly to be considered as inflammation of the brain. This, however, is a mistaken view of the subject. The term *idiopathic phrenitis* has been applied when the disease arises *primarily*, and that

either spontaneously or from some evident external cause: such as *exposure to cold*, *insolation* &c.; while the disease has been called *symptomatic*, when it comes on *secondarily* in the course of other diseases, as *fever*, or any other. This distinction, however, is a frivolous one. The disease is not the less inflammation of the brain, because it occurs in the course of another disease, which may be then considered as its *occasional* or exciting cause. Undoubtedly, the treatment called for in such cases, may sometimes require modification; not because the disease in itself is different, but because it arises in a different state of system, and under different circumstances.

The *progress* and *termination* of *phrenitis* are various, according to its violence, and the greater or less disposition there may be to disease in the brain. Many cases terminate speedily, and favourably; others proceed rapidly to a fatal termination, by interruption of functions. Or it may go on to *suppuration*; though this is only likely to happen in the milder and protracted forms of the disease. It may end also in a rupture of blood-vessels in the brain, and thus give rise to *apoplexy*, from extravasation of blood. Or, lastly, it may terminate in *insanity*, or permanent derangement of intellect, from the alteration of structure induced by the inflammation.

For the *causes* of *phrenitis*, I must refer you to a former lecture, where I pointed out the causes of inflammation of the brain altogether; for they are not peculiar in this case. Nor are the appearances on dissection such, as to enable us to discriminate between this and the other forms of inflammation of the brain, whether *acute hydrocephalus*, or *idiopathic fever*. The most speedily fatal cases, in all the varieties of the disease, are those which leave the fewest and faintest traces behind them.

Treatment of phrenitis.—The treatment of this disease is to be conducted upon the common principles that govern our practice in other inflammations: that is, according to the degree and stage of the complaint, and the habit of the patient; with an attention to such points as experience may have taught us, arising from the peculiar nature of the organ affected. And I must here repeat to you what I formerly remarked, that you are not to form your indications of cure, as is often done, from the *state of mind* of the patient, but from the ordinary bodily signs, as in other inflammations. You are not called upon, for example, to bleed to an extreme degree, every case that is attended with active or even furious delirium; nor must you stimulate, merely because the spirits are low and depressed: but first endeavour to ascertain the bodily strength, by the ordinary means.

In the treatment of *phrenitis*, your first attention should be paid to the *causes* of the disease; in order to attempt their removal, if they should be still acting. This being accomplished as far as possible, the usual remedies for inflammation are to be applied. If the case is recent, and the patient strong, large and repeated bloodletting will be generally required: in weak subjects, the quantity of blood to be taken must be governed by the bodily strength, and not by the violence of the delirium. You are not to consider *opening the temporal* artery, as having any peculiar advantage over *venesection*; but rather the contrary, as I before stated. *Cathartics*, and the *digitalis* in minute doses, are equally indicated as in other inflammations. *Blistering*, where the pulse is much hurried, appears a doubtful remedy; as is the application of a *blister* to the scalp. It is generally better to apply continued *cold* to the head, the good effect of which I have sometimes thought was enhanced, by the occasional use of warm fomentations.

There is a period when *opium* appears to be extremely useful; but its too early application, for the mere purpose of procuring sleep, is injurious and even dangerous. When *bloodletting* has been as largely used as the circumstances of the case appear to warrant, then and not till then is *opium* proper. Under this limitation, it often produces the best effect, by conciliating sleep, during which there is often a gradual and sometimes an entire subsidence of all the symptoms; so that the patient, after a sleep of several hours duration, awakes free from delirium, and becomes immediately convalescent. Whether these good effects are most likely to be produced by small and repeated doses of the remedy, or by a single large dose, it is difficult to determine. I am rather inclined to prefer the latter mode; giving, for example, two or three grains of the solid opium, or 40 or 50 drops of the tincture, at a dose: but I have also seen equally good effects from the quantity of five or six drops, given repeatedly at intervals of a few hours. The combination of *opium* with *ipecacuanha*, in the form of *Dover's powder*, appears well adapted to the case.

There is one variety of *phrenitis* that has of late been much talked of, under the novel designation of *delirium tremens*; as if it were a new or nondescript disease; wearing the semblance only of inflammation of the brain, but not actually such. And accordingly a peculiar mode of treatment has been suggested, in a great measure, the reverse of that which is usually employed in *phrenitis*; namely, large and repeated doses of opium; *bloodletting* being altogether reprobated in these cases.

Now I think it right to guard you against an indiscriminate admission of these opinions, though I must allow they come from a respectable quarter. The chief peculiarities of the disease are, the muscular tremor of the hands that attends the delirium, and its taking place mostly in habitual drunkards. But the disease itself is nevertheless inflammation in the brain, though its character may be modified by the nature of the exciting cause, and the habit of the patient. In many of these cases, undoubtedly, the general strength is much impaired, so as not to call for evacuations, especially of blood. But this rule is far from being general. *Delirium*, with muscular tremors, is often met with, where the habit is still strong, and where there are sufficient marks of a very active state of disease going on in the brain. In such cases, though produced by frequent intoxication, *bloodletting* is as necessary, though not perhaps to the same extent, as in ordinary cases of *phrenitis*. After the abstraction of blood, *opium* may be safely resorted to; though I believe it is less necessary than is commonly imagined. I have several times had to regret the neglect of bleeding in these cases; and have been compelled to resort to it, from the failure of other means.

The last form of *acute* inflammation of the brain, which I shall have to notice, is that termed *idiopathic fever*, a subject that will require a good deal of attention, both from its extent, and from the diversity of opinion that exists with regard to its intrinsic nature. This I shall resume at our next meeting.

FOREIGN DEPARTMENT.

Clinical Report of the Practice in La Charité, during the Winter of 1825-6.

It is the bounden duty of every Hospital Surgeon and Physician to publish, from time to time, detailed statements of the results of his practice, and thus give the public and the profession the benefit of his experience. The truth is so obvious, that it requires no argument to enforce it. Some time since, when looking over a work on the diseases of the brain, our attention was arrested by a note at the bottom of one of the pages, in which the author said that he had consulted (on some of the points treated of in his book) his friend Mr. ——, who had great opportunities of observation; that he had dissected the brains of some hundreds of lunatics and idiots; and that he had found—no matter what he found—he had opportunities, or rather they were thrown away on him, for he does not appear to have even taken a note of the examinations, or to have inquired into the histories of the cases, and but for a casual note in the book of his "confrere," the world would have been ignorant that such a mass of apathy and negligence had ever existed. But we should not, probably, comment hardly upon this gentleman; his case and his conduct are by no means singular; they are in strict conformity with that of many, nay, most of the hospital practitioners of the country, more especially those who complained most loudly of the invasion of *their private* rights by the publication of reports of cases which are treated in the *public* hospitals. Where are we to find the results of the practice of our leading physicians and surgeons? In the pages of The Lancet; and but for its exertions they would have been consigned to utter oblivion. About 70,000 patients annually pass in review before the medical staff of the great hospitals of London, and when we come to inquire what new facts are elicited? what difficulties are solved? what improvements are made? in a word, when we ask, what are these gentlemen doing?—we are obliged to pause for a reply; and in the end, most probably, receive none, or next to none—for it can scarcely be called a reply to say that certain purists (both surgeons and physicians) have lately got into the practice of sending reports of cases to the *Yellow Journal*—as they are drawn up in such a way as to render them unavailable for any useful purpose, and the practice pursued in several of them is such, that we devoutly hope it may never be drawn into a precedent for the guidance of others.

We want in our hospitals a regular system of clinical instruction, in order to render students fitted for the active duties of their profession, and that they may learn its principles and practice at the bed-side of the patient, and not by books, or by mere routine courses of lectures delivered at a distance from the hospitals, and without any reference to the cases which they contain. We want to see a system of discipline established, which may teach young men how to observe, to think, to investigate; and this can only be done by directing their attention to cases, pointing out the leading characters of different diseases, indicating the methods of distinguishing them, and, finally, by marking those methods of treatment which have been sanctioned by experience. This, or some such plan, should be carried into execution; students require it as an essential requisite for completing their education. We believe they are usually, on admission into the hospitals, required to pay a pretty large fee, for which they receive no

other return than the privilege of "walking the wards" for a certain number of hours each day, during some prescribed period. This certainly is not as it should be; students pay their money, and are thereby entitled to receive an equivalent in the way of clinical instruction, from those into whose pockets that money goes. A few lectures given occasionally cannot be considered an equivalent. Nothing short of a regular system of clinical instruction can be considered as adequate. When this view of the subject comes to be adopted by the students, and those interested in their welfare, the improvements here pointed out will speedily be enforced, for their reasonableness cannot be denied.

These remarks very forcibly occurred to us on looking over the last clinical report drawn up by Professor Laennec. It was published but a few months before his death. During the half year ending May 1826, 188 patients were admitted into the clinical wards of the hospital "La Charité." And, as may be supposed, they presented almost every form of disease. The proportion of deaths appears to be much greater than that which occurs in the hospitals in this country. In acute cases, the proportion of deaths to recoveries, was as one to eight, and of chronic cases, as one to four. Though Laennec for a long time supported the doctrine of the essentiality of fevers, or, in other words, that fevers consisted in a general disturbance of the whole system, without being connected with, or dependent on, any particular lesion of any part or organ; he appears to have given up that dogma latterly, and to have admitted that "some of the cases of fever, treated during the previous half year, were accompanied by symptoms of intestinal irritation, though the greater number were free from them." We believe, that had his mind not been occupied by a particular prepossession on this subject, and had not that prepossession been strengthened by a spirit of opposition against the dogmas of Broussais and his followers, he would have seen this subject in its true light, and have been forced to admit, that the majority of fevers no matter what their denomination, may be connected with, and probably dependent on, gastro-intestinal irritation.

The most frequent complication in the fever cases referred to in this report, was catarrh, of varying degrees of intensity. During the first days this could be distinctly recognised by auscultation, long before it presented any of the ordinary symptoms which are said to mark that affection. It was only towards the middle period of their progress, that these symptoms made their appearance. It is very remarkable, that almost all the fatal cases presented a few days before death, symptoms of pneumonia, no matter under what form of disease they had laboured; towards the posterior part of the lungs, the "crepitous wheeze" could be discovered by means of the stethoscope. To this complication, Laennec applied the term "*peripneumonie des mourans.*" This appears to have been altogether produced while the patients were in *articulo mortis*, and to have depended on the posture in which they lay, viz. on the back, causing, in fact, a gravitation of the blood towards the more depending parts. This mode of reasoning appears to be borne out by a case which the Professor had an opportunity of examining. A man labouring under fever of a very aggravated character, was attacked with phagedena and sloughing of the nates, in consequence of which he was laid on his face and hands for some days before his death. On inspecting the body, the anterior part of the lungs was found to be the seat of this cadaveric pneumonia, if it could be so called. For our part we readily admit that this gravitation really takes place in the lungs, and that it may be characterised by symptoms similar to those which mark œdema and those organs, but not by those of pneumonia; we more than doubt whether it can present, either during life, the symptoms of true pneumonia; or, after death, the anatomical characters of that disease. The condition of the part in these two affections, must be altogether different; the one consists in an inflammatory action going on during the life of the individual, and as such may be termed a vital process; the other, according to the explanation given of it, commences when life is at its last ebb, and when the fluids of the body, in some degree uncontrolled by its power, begin to obey the influence of their own gravitation, and subside towards the more dependent parts.

The supporters of auscultation, the admirers of its minute divisions and nice distinctions, may derive from these reports many strong arguments in support of their favourite hypothesis. For here we have cases, in which the existence of catarrh was ascertained by the stethoscope, before any symptoms arose which could have induced even a suspicion of its presence, and pneumonia was also indicated before any of those signs arose, which are set down in systematic words as characteristic of it. Doubtless we shall, by and by, hear it asserted, that any student who has "cultivated his ears" a little, as they phrase it, will be able to distinguish diseases far better than the seniors who plume themselves so much on the "tactus eruditus," which they have acquired by feeling the skin and the

pulse, and inspecting the tongue, for half a century.

The treatment pursued by the French Professor, is not likely to be adopted by any of our practitioners. He confined himself, as he said, "to simple expectation." The expectant medicine was once well called, a meditation on death. However, in some few young and plethoric patients, local and general bleeding were resorted to—but neither tonics or purgatives were ever employed. "Les toniques, et les purgatifs n'ont point été employés parce que l'occasion ne s'en est presenté." Laennec it should be recollected, was a religious believer in the doctrine of crises, and of critical days; hence he would not employ purgatives, lest they should interfere with the salutary effects of Nature. He pursued his expectant practice, waiting to see how Nature should proceed with her operations; and if he could not catch her at her work, he simply looked on, applied his stethoscope now and again, took a pinch of snuff, and—walked on.

He had rather a curious notion of the effect of purgatives, he thought that in some cases they tended to induce ulceration in the intestines to heal, hence he was induced to recommend the use of them towards the close of the disease. Some individuals assert that ulceration in the intestines in fever cases is produced by purgatives, during the course of the disease, inducing inflammation; but here we have instances of ulceration occurring in the intestines of patients who never took a single dose of any purgative medicine—consequently, this dogma is not well founded.*

Amongst the cases of the nervous system, there were two instances of softening of the cerebral substance: in one, it occupied the right lobe of the cerebellum; in the other, the centre of the middle of the right hemisphere. In the latter case, there was an evident contraction in the right arm, the left not having been at all engaged. It is usually said, that affections of one side of the brain influence the limb of the opposite side; here is an exception to this rule, cited by Laennec;—there were seven of a similar description reported by Bayle in the *Revue Medicale* for January, 1824.

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* The consequence is not very evident.—Ed.

TO

JOHN ABERNETHY, ESQ. F.R.S.

Quod verum atque decens, curo et rogo, et omnis in hoc sum.

Sir,—As you have become of late a kind of Christian advocate, as well as a surgical lecturer, and have apparently endeavoured to add to your long-established reputation as a teacher of anatomy, by assuming the higher functions of a teacher of morals, I trust that the following observations will neither be deemed impertinent when submitted to the serious consideration of yourself, nor irrelevant as applied to the conduct of a certain surgical party with which you appear to have got connected. In order to explain to you more fully what I mean, I must refer you to certain opinions which you have maintained of late, in a sort of theological controversy which sprang out of your Hunterian Orations, in which you, after having perverted and misrepresented Mr. Hunter's opinions respecting life, not only hastily adopted them as your own, but founded thereon an extraordinary physiological sophistry—gave to your sophistry the appellation of a Christian doctrine, and, in the true Protestant spirit of a newly modelled sectarian, you opposed, even to persecution, those who differed from you in sentiment.

Your opinions on the subject of Body, Life, and Mind, would have passed with me for the mere vagaries of a man of genius, had you not assumed for them the important character which belongs alone to the precepts of religion. I recognise no doctrine as Christian which does not emanate from the authorised expounders of the Bible.—In every fresh instance of Protestant sectarianism, from the first heresy of Luther and the blasphemous Calvin, down to the spurious christianity of your dissecting room, I have been enabled to detect the *cloven hoof*; they have all wanted that distinctive mark of the true faith which, in ordinary discourse, is called liberal, generous, and humane conduct, and which, in the emphatical language of religion, is designated as the *fruits of sanctity*. Had you stated your opinions concerning Life, and your *reasons* for entertaining them, with that deference which belongs exclusively to genuine merit, and had you extended toleration to those who differed from you in opinion, these observations would probably have never issued from my pen. But when you illiberally assured your opponents that their holding opposite opinions from yourself arose from their own immoral conduct, and described a modest scepticism of your phy-

siological religion, on the part of your adversaries, as a base endeavour to "pluck the fair rose of innocence from the bosom that it adorned, in order to place a serpent there," in utter defiance of the divine command, "*judge not that ye be not judged*," then it became advisable to question the validity of your doctrines, and to see, and to make others see, how far they really tended to impart to those who embraced them just and honourable sentiments, such as make good members of society and worthy disciples of the Hunterian school.

At the time you put forth your religious opinions a pamphlet appeared, entitled, "Somatopsychonoologia," which exposed the sandy foundation of your heresy, and predicted its incapability of ensuring to its victims that fine and generous morality, of which you described it to be the only genuine fountain. I propose to try how far this prediction has been fulfilled by a critical examination of the subsequent conduct of yourself, and of others of *your* "party," who have embraced your notions and supported your *sect*! And turning to the pages of THE LANCET, I read an account of the noble, open, "straight forward," and honourable transactions which were discussed in the theatre of St. Bartholomew's Hospital, on three several days of late; which ominous days ought to be recorded in red letters, in the calendar of Hunterian christianity!!! I appeal seriously to your judgment as a man of common sense, and to your candour as a gentleman of common honour, whether, when the editor of THE LANCET* publicly dragged forth the dramatic scenes of those three days from the imagined privacy of a lecture room, and planted a watch over the conduct of their actors, he "tore the rose of innocence from the bosom it adorned, to place a *serpent* there?" Was it the Love of God, or the Lucre of Gain, that suggested the *apparent* expedient of advertising Mr. Stanley, and substituting Mr. Skey? I will not take on myself to answer this question, nor illiberally measure out that *judgment on the* MOTIVES of others, which you would have me to believe will be measured to me again by a tribunal which alone can judge of the secrets of hearts; but as far as my short-sighted views of things will enable me to *judge of* ACTIONS, and to decide the palm of victory between two contending parties, I give it *not* to the Hunterian school, but, on the contrary, I say as I did when I read the Numbers 169 and 170 of THE LANCET, in allusion to the three days adverted to, "If this be the fruits of a belief in '*matter, vitality, and intelligence*,' may God of his mercy grant that all my friends come over to the ensigns of *materialism*." Disgusted at the gross inconsistency above alluded to, I am induced to request you, for the future, to abstain from any religious effusions in your medical works, and to urge you, as charity begins at home, to reform your own sect, before you attempt to reform the world. I have always believed you to possess too good a character as a citizen, and too extensive a reputation as a physiologist, to deserve the imputation of disingenuousness, which many will be apt, however undeservedly, to fix upon you, if practice hereafter shall not ostensibly take the lead of precept; and if the instantaneous abandonment on your part of an illiberal party of surgeons does not speedily become the just penance you perform, for the shock which was offered to the feelings of your many enlightened and liberal friends, when THE LANCET held up to their view those transactions to which a deep sense of justice has obliged me to make a delicate but yet painful allusion.

I shall now address you on the subject of the party of surgeons to which I have adverted. It is well known that a certain number of that profession united their consummate wisdom, in a certain meeting convened for the purpose of repressing the activity of THE LANCET; and it is equally well known that the labours of their joint wisdom, on this occasion, have been totally unavailing. These surgeons must be either very ignorant of the consequences of taking this step, or very stubborn in pursuing the measure when rashly undertaken, or they could not have so effectually injured their own cause. What can weaken the confidence of a discerning public in the integrity of the medical profession, so much as an attempt on the part of the practitioner to conceal his professional acts from public view? If I were asked to whom I would by preference entrust the care of my animal fabric, in the case of any critical disaster, I would reply—To him who courts, and not to him who shrinks, from the tribunal of public opinion. No man has a right to perform an operation in the corner of an hospital, which he would object to perform in the corner of a street. I have been sojourning so far from the scene of action, that I do not pretend to know whether you were one of the number; but if you were, you have not done justice to the liberal character which you enjoyed some few years ago. You have, however, been misled into one act, which is quite inconsistent with the liberal precepts of the religion that you deem your efforts necessary to defend, and which the public prints convinced me of, by the report they recorded of your action in Chancery against the publisher of your Lectures. That you, Sir, individually, will never seek an occasion to conceal an

* See LANCET, Nos. 168 & 169.

operation, I feel persuaded. You have always said so much in your lectures and conversation, if I be rightly informed, against the wickedness of an unwarrantable *experiment* in surgery, that to suppose that you would object to any fair reporter being present in the theatre of operation, would be to insult your integrity, and impeach the veracity of your discourse. You have also spoken formerly of the expediency of a sort of quintennial examination of surgeons actually in practice, in order to satisfy the public that they had suffered nothing from the tottering decrepitude of advancing senility, and were still *bona fide* capable of doing all those operations for which the public might call on them. With these sentiments, Sir, fresh in the memory of your quondam pupils, how can you lend your aid to the cause of a parcel of "hole and corner" practitioners, whose only imaginable object for secrecy must be a consciousness either of unwarranted temerity in experiment, or of ignorance of the nature of the operations that they undertake to perform? It has been always so much the custom of medical men obtrusively to clog the public press with the most trivial case of any imagined importance which they have undertaken, as a means of acquiring renown and practice, that a studied endeavour to withhold their conduct in hospital cases from general view, must engender on the part of the public a shrewd suspicion, that hospital practice is radically faulty. I am told, that there is a society or club of surgeons, formed or forming, who have excluded from their number those enlightened and able practioners who refuse to lend their aid to this work of concealment. If this be true, it is only just that the public should know their names. I pity them, *for they know not what they do*. But I hope, out of the respect I bear to the memory of your former character, that you have not *now* compromised it by becoming one of their number. They may *meditate* the destruction of THE LANCET, and *effect* their own. The sword that was unsheathed for murder, has more than once become the instrument of suicide.

I remember some time ago, many surgeons in London incurred the most severe censure from the public press, for certain cruel experiments on animals; and a forcible appeal was made to the public, never to entrust the lives and limbs of human beings to men who, following the silly example of Mr. Hunter, had destroyed some thousands of dogs, cats, pigs, and rats, by experiments so cruel, that their perpetrators ought not to be trusted with the temptation to do similar acts of experimental barbarity on their patients.

I admitted the justice of the remark at the time, and I said, these surgeons who sought to find out the laws of life, by such means, were as bad as Prometheus, who tried to draw down the fire of animation from heaven to make a man with; and like Prometheus they will be punished with the vulture of disappointment. But if there be among the "hole and corner" fraternity, any men who have done such acts, their desire of concealment will be construed into a desire of concealing cruelty or folly in the practice of their profession.

I am informed that the anti-liberal party, as they claim a right to be called by stigmatizing the opposite party as *liberal*, are very religious; that many of them go regularly to church, to meetings, and to conventicles; some make evangelical sermons in consultation; and others distribute the bible, and style themselves "followers of the meek and lowly Jesus."

Sir, the Devil's darling vice is the pride that apes humility; and the father of lies is never so well pleased as when he can throw the mantle of hypocrisy over the mammon of unrighteousnes, and, with a Satanic smile, send forth wolves in sheep's clothing to seduce the unwary. *By their fruits ye shall know them.* From these things, and from all that I have seen of *heretical* christianity, whether clerical, methodistical, or medical, I pray heaven to deliver me — Atheism itself might rejoice at the weakness and frailty of her opponents, and gladly giving up the contest, might despise rather than mainfain the guerrilla warfare of such maudlin foes.

The cant of the times is a subject of regret, but that it should pervade the medical profession, is a disgrace. I understand that you talked in a very religious and benevolent way of *him who healed the sick*, and that your affectionate addresses to your pupils, are affecting from their flow of evangelical pathos. I repeat it, that you had better leave off this unprofitable jargon. Who could endure to hear the master of a *slave ship in a storm* appeal to *him* who walked the waves? You have frequently discoursed, in the hearing of your pupils, of the superior efficacy of those religious physiological opinions which have a tendency to *unite men in the "bond of religious obligation."* Now I ask you, whether you think such opinions of so much importance to mankind that you are *bound*, in *conscience*, to secure by BOND, to all future classes of anatomical pupils, a perpetuity in the advantage of hearing such opinions inculcated and made a part of the anatomical course? If you do thus feel bound in conscience, why it would no doubt justify you, in your own opinion, for an attempt to secure by BOND the future situation of lecturer exclusively to the person who is the most likely to transmit your religious opinions, and to entwine them, as you have done, with the lectures. There is a

report of a bond of this kind having been made; is it true? and if true, are the medical public to consider it as a *case of conscience*, and to refer it to the operation of a belief in the existence of an immaterial principle of intelligence? If this be a case of conscience, allow me to suggest a subject for conscientious reflection:—whether the *bonded* expounder of the theory of life be capable also of teaching the anatomy of the body? and whether such a deep knowledge of the spiritual principle as you possess, furnishes you also with a prospective knowledge of the qualifications of a future lecturer now probably in embryo? Of the advantages that may accrue to yourself from such a bond, I say nothing; but, as you have descanted "on the advantages of a Christian's hope," I may take the liberty to remind you, that the advantages of every hope must depend on the fulfilment of the conditions of its realization. Christianity says, *freely have ye received—freely give*; and assures us, that *it is easier for a cable to go through the eye of a needle than for a rich man to enter the kingdom of heaven*. You formerly held this principle most sacred, and waged a sort of war against the crafty and acquisitive policy of certain elderly surgeons of the hospital, and you used to say, that you thought their eligibility to office ought to be limited by age, and that sixty years should be considered as superannuation. Have you found reasons for changing your sentiments, in the conscientious fear that younger surgeons might infuse into pupils' minds some of the principles of materialism, so prevalent among the rising generation? I am ignorant of the solution of these questions, and seek to be informed of them before I make up my mind as to the tendency of your opinions on "Body, Life, and Mind;" because I prefer the genuine rule of judging of every tree by its fruits, since the fruits afford the only safe clue to the character of the sap.

In short, Sir, as cant is going out of fashion, lay cant aside; for even as good wine needs no bush, nor native beauty the foreign aid of ornament, so neither does sound morality seek a trumpeter in evangelical ejaculation, nor sound medical principles and practice require the superfluous support of any hypothetical notions respecting the origin of Life.

Trusting that you will take these hints, and leave off canting, a practice an authority you own declares to be inefficient, you will continue to pursue, undescanted on, the path of rectitude which alone leads to success.

CRITO.

London, Dec. 1826.

ST. THOMAS'S HOSPITAL.

CLINICAL LECTURE

BY MR. GREEN.

November 7th.

THE subject of Mr. Green's Lecture today, was on the operation of laying open the urethra in the perineum; and he commenced by enumerating the circumstances under which this operation is necessary.

There are some cases of retention of urine, said Mr. Green, in which it is a far better plan to make an incision into the membranous part of the urethra, and by this means introduce an instrument to relieve the bladder, than to make an opening into that viscus. But it is only when the obstruction to the passage of urine exists in the perineum, that such a mode of practice can be successfully adopted; for retention of urine may arise from calculus, or from enlarged prostate, or other causes, which are beyond the seat of the proposed operation. It is proper to lay open the urethra in some cases of permanent stricture, and I prefer this mode of practice to the use of the caustic bougie. In those cases in which stricture of the urethra is complicated with abscess, or in which, after the healing of the abscess, a fistulous opening remains in the perineum—these are cases in which it is sometimes proper to perform the operation. In rupture of the urethra, when from a person falling across a hard body, the edge of which, for instance, strikes in the perineum, and occasions a laceration of the urethra. Here, again, the measure to be pursued, is laying open the canal, in order to relieve or prevent the effusion of urine which in general succeeds to this accident.

You will perceive, then, Gentlemen, from these remarks, that the operation is performed under various circumstances. But I would observe to you, that it is not a matter of indifference how the operation is performed. The common way is to proceed as in the operation of lithotomy, placing the patient precisely in the same position, and making an incision between the bulb and crus penis. This is, however, a different business to the operation for stone, where, after having cut into the urethra, you find a groove in the sound, into which the point of the instrument is put, and is then readily carried onwards to the bladder. I have seen great difficulty experienced in finding the urethra, when an attempt has been made to lay it open, by making an incision between the bulb and crus penis; and I therefore preprefer making it directly in the line of the raphè. The extent of the incision must be

regulated by the concomitant circumstances; as, for instance, when there is large abscess or diffused extravasation of urine, it may be necessary to lay open the back part of the scrotum. There is no question, that cutting in the line of the raphè facilitates the finding of the urethra, because it is directly under the raphè, whether the operation be performed under the symphisis pubis, or some way further backward. But there is an objection, you will perhaps say, to dividing the bulb; I know of no mischief resulting from it. There is certainly less risk of active hæmorrhage from the wounding of a large artery, than if the operation were performed on the side, because the large arteries are there situated. You might divide the artery of the bulb, but it is not to be expected that, even with moderate care, you would cut the trunk of the pudendal artery. You should, however, bear in mind, that it becomes superficial at the arch of the pubes, where about to divide into the deep-seated and dorsal arteries.

In performing this operation of laying open the urethra from the perineum, you will then, as I before observed, place the patient in the same position as in the operation for stone; that is, so as to have the perineum fairly before you. Pass an instrument down the urethra to the strictured portion, then make your incision (varying its extent according to circumstances) in the line of the raphè, and having laid open the urethra, withdraw the sound, and pass a catheter down from the extremity of the penis. The point of the catheter will issue at the orifice in the perineum, and it will then be necessary to find the lower portion of the urethra, in order to carry the instrument onwards to the bladder. If you experience any difficulty in finding the urethra, a probe may be used. On most occasions it is necessary to retain the catheter in the bladder, during the process of granulation in the wound of the perineum. The instrument should be of a firm nature, and I prefer a silver catheter, retained in the usual manner by tape.

When there is extensive destruction of the soft parts in the perineum, consequent upon extravasation of urine, it is unnecessary to introduce the catheter at first, until the sloughs have separated, and granulation has commenced. The pewter catheter, in some cases, is to be preferred to the silver one; it admits of the extremity being bent, so that it may be worn in the bladder without the use of tapes or bandages to retain it. I think the pewter instrument more particularly useful in those cases where the prostate is enlarged, and I have known it worn for many months without inconvenience. A catheter made of caoutchouc is useful, inasmuch as it is flexible; but, being elastic, it too readily quits the bladder. There is a contrivance for fixing the catheter in the bladder, for which, I think, we are indebted to Sir Everard Home: the instrument is made by Weiss, and it consists of an elastic wire collar, which is put on the penis; tapes are fastened to this collar, and from thence to the rings on each side of the catheter.

Having explained the nature of the operation of laying open the urethra, and the circumstances connected therewith, you will be better able to understand the treatment adopted in the cases, which I shall now speak of; and the first is a case of *ruptured urethra**.

The patient, a middle aged man, was admitted on the evening of the 10th of Aug. He had fallen with his legs asunder, across the edge of a boat, on the morning of the 8th, and thus received a violent blow in the perineum, which lacerated the urethra without producing any external wound.

Complete retention of urine was the consequence of the accident; the man had emptied his bladder a short time previously, but was not able to expel a single drop afterwards, up to the period of his admission—an interval of nearly three days. The bladder, therefore, had become much distended, and very painful. Attempts were made to pass the catheter, but failing in this, an incision was made in the perineum, along the line of the raphè. The integuments and structure beneath being divided, and the clots of effused blood removed, it was found that the bulb was extensively lacerated, and as the injury extended under the arch of the pubes, it was evident that the triangular ligament was also torn. A catheter was passed from the penis downwards, and the point of the instrument made its appearance at the orifice in the perineum; there was much less difficulty experienced in finding the lower portion of urethra than might naturally have been anticipated, from which circumstance it was inferred that the urethra was torn through, near to the face of the prostate gland, and that consequently the lower portion retained its natural situation. Three pints of urine, tinged with blood, were drawn off, and a

* This case may be found copiously reported in No. 159, Vol. X. of THE LANCET; it is therefore only necessary to recapitulate the leading facts. We have occasion to know, that the person who *furnished* Mr. Green with the particulars of this case for Lecture, was mainly indebted to the report in THE LANCET. So much for the competency of the "*authenticated*" Hospital reporters; they are already complaining of the paucity of the *Yellow Man's* pay.

silver catheter of large size was left in the bladder. There was no untoward symptoms succeeding the operation, and ten days afterwards, the catheter was removed and cleaned, and again introduced without difficulty. From peculiar circumstances, the patient was compelled to leave the Hospital on the 6th of September; at which time the wound had not quite healed, and the catheter was still retained in the bladder.

Now, continued Mr. Green, it is somewhat curious, that although the urethra was torn through, no extravasation of urine had taken place, for in the cells of effused blood in the perineum into which I cut, there was not a drop of urine. The only way of explaining this, is by supposing that the pressure of the extravasated blood in the surrounding loose cellular membrane, prevented the flow of urine through the urethra.*

You will see, Gentlemen, in this case an illustration of the good effects of the operation I have described; and I will now speak of a case in which it was performed, in consequence of a *fistulous opening in the perineum, accompanied with stricture of the urethra.*

The patient was 53 years of age, he had a fistulous opening in the perineum, and also at the lower part of the scrotum, through which the urine constantly dribbled, whilst from the natural passage, it only passed in small drops. He had stricture of the urethra, which was of 20 years standing; the fistulous openings were of 15 years duration, and occasioned by an operation performed for retention of urine. The integuments of the scrotum were much enlarged, thickened, and irregular—in fact, they were almost cartilaginous; I foresaw, from the first, that this was a hopeless case for the introduction of bougies, and I therefore recommended an operation. For this purpose the patient was placed in the position I have described, and I commenced by making an incision in the perineum. Here a circumstance occurred worthy of note:—whether it was that my assistant varied the instrument, or whether it arose from the thickened and diseased state of parts in some measure deceiving me, I know not; but, however, I deviated from the direct line of the raphè, and from this cause a pretty considerable stream of blood followed the incision—a stream equal in size to that which follows an opening of the temporal artery.

This hæmorrhage occasioned considerable delay in the operation; it was a matter of doubt to me what vessel was wounded, but I directed pressure to be made in the direction of the pudic artery, which restrained the bleeding, and the pressure was continued for about two hours. At length I proceeded with the operation, fairly laid open the urethra, and introduced a catheter into the bladder, from the penis downwards. The operation was performed on the 28th of July. On the following day, the man was free from any unfavourable symptoms; but, on the 31st, there was considerable febrile excitement. On the 2d of August the catheter slipped out of the bladder, and I was unable to reintroduce it, the pressure of the point of the instrument, in the perineum, occasioning severe pain. Two days afterwards, however, by first making use of a female catheter from the wound, I was enabled to pass the male instrument. I should observe to you, by the way, that the prostate gland was much enlarged. There was nothing particular occurred after this period; the catheter was withdrawn at the end of three weeks for the purpose of cleaning it, and was again readily introduced. The last report on this case is dated Sept. 5, when it appears the patient was walking about the ward, and shortly after was dismissed quite well.

There are two cases in Job's ward, admitted on the 12th of October, to which I shall next direct your attention. The first patient had a large abscess in the perineum, but there was no extravasation of urine, and I therefore simply made the external incision. In the course of a day or two, a small quantity of urine passed through the wound; but as soon as the process of granulation commenced, the flow of urine ceased, and it was therefore considered unnecessary to introduce the catheter. But you must bear in mind, that in this case there is no stricture in the urethra.

In the next case, a patient of the name of Casey, I was a little deceived, and it may serve to show you the necessity of a strict diagnosis. When the man was admitted, he complained of pain and tenderness about the fore-part of the bulb, also extending up the cord. I conceived it probable that an abcess was forming in one of the mucous glands of the urethra, or in its immediate vicinity. Leeches were ordered to be applied to the perineum, and the patient to keep his bed. On the following day the scrotum was somewhat swollen, and there was a slight blush of inflammation about it;

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* In No. 162. Vol XI. of THE LANCET, a second case of laceration of the urethra is reported, in which also there was no extravasation of urine. The patient was under the care of Mr. Travers, who, on being questioned by a pupil as to the cause of non-extravasation of urine, replied that he was not quite sure "*whether it was functional or physical!—but it certainly was a wise provision of Nature.*" Bravo! Benjamin.

this led me to inquire whether any difficulty had occurred in passing the urine, the answer to which was in the negative. From this circumstance, together with the absence of swelling in the perineum, I was led to infer that no extravasation of urine had taken place. But the next day, on visiting the patient, I found there was most extensive extravasation of urine; the perineum had become swollen, and the scrotum had the true characters which denote the effusion of urine into its cellular substance; it had a dark red unhealthy appearance, which extended along the chords to the groins, and also to the lower part of the abdomen. It was now evident that the proper plan would have been to have performed an operation for laying open the urethra on the day before, for at that time extravasation had commenced; but I was deceived in my opinion. The patient, on my proposing to operate, readily assented, and I therefore proceeded in the usual manner. Since the operation, sloughs have separated from the perineum, groin, and pubes, and the wounds have granulated. It will now be necessary to introduce a catheter into the bladder, and let the wound heal over it, so as to re-establish a passage for the urine.

Mr. Green has this season adopted what appears to us a very useful plan; after each Clinical Lecture he appropriates a short time for discussing the subject on which he has previously lectured; a kind of surgical *conversations* between himself and the pupils.

At the conclusion of the Lecture to-day, a pupil inquired of Mr. Green whether, if the bulb were divided (as it would be in the manner proposed by Mr. Green for laying open the urethra) there was not danger of the patient becoming impotent from the imperfect erection of the penis. To this question Mr. Green replied, that he had never seen such a result occur from division of the bulb; but he thought the objection (which to him was novel,) seriously worthy of consideration, and more especially if supported by any facts. A gentleman present spoke of a case of priapism that occurred in the practice of Mr. Callaway, in which an incision was made into the bulb, after every other means had failed, and the disease had continued sixteen days. The man, he said, had remained impotent from that period.*

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* This case was detailed (consigned to an untimely end) in the Medical Mausoleum for April 1824. Jemmy Johnson copied it into his Quarterly Periscope, entitling it "*Mr. Callaway's case of Priapism.*"

ON THE USE OF THE BLISTERER,

By Sir Anthony Carlisle.

The important difference between a scald and a burn, which I wished particularly to point out in my letter to Sir Gilbert Blane, is this—a simple scald, arising from the momentary application of boiling water, ought to be limited to that inflammatory excitement of the skin which produces a discharge of serum under the scarf skin, and this effect closely resembles a blister occasioned by cantharides; but in the case of a burn, the higher degree, or longer continuance of heat, corrugates the true skin, and thus its vascular structure is spoiled, and an eschar, or a local detachment of the deadened part, is the consequence.

When a mere blistering effect is desired, the operator must consider that the intervention of wetted silk presents the medium of water several degrees below the boiling point, as the agent to act upon the living skin; and if the duration of the contact resembles the sudden effusion of scalding water the results are similar. Respecting the slow and longer continued action of common blisters, I think that every medical man will admit that when counter irritation is required, the more sudden and intense the diversion, whether from the nervous or the sanguineous systems, the greater will be the power obtained, and thereby a more rational chance of relief from disorders of less degree. I apprehend that the remarkable cures effected by the moxa cautery depend much on the violence of its action, because in those instances where the metallic conductor of heat has acted as an escharotic, the remedy has proved most efficacious.

A gentleman, who had resided many years in Tanjou, informs me that the actual cautery is a very ancient remedy among the Hindoos for diseases of cattle, and that a cow, or an ox, is rarely seen in that part of India without numerous scars of the cautery upon its body. The success of this practice is so firmly established, that the natives resort to the cautery whenever the hide of an animal appears to be tight or rough, and the application is usually made along the side of the spine.

The repugnance of patients to a heat-blister may be easily removed, by judiciously representing the momentary continuance of the pain, and the probable and speedy alleviation of a serious disease.—*Annals of Philosophy.*

THE LANCET.

London, Saturday, Dec. 23, 1826.

1.—*A Short Inquiry into the Capillary Circulation of the Blood; with a comparative View of the more intimate Nature of Inflammation, and an Introductory Essay.* By JAMES BLACK, M. D., S. R. N., and Member of the Royal College of Physicians of London. 8vo. pp. 176. London, 1826. Longman and Co.

2.—*A Comparative view of the more intimate Nature of Fever; deduced from Physiological Analysis, and illustrated by Critical Remarks and Practical Observations.* By JAMES BLACK, M. D., S. R. N., &c. 8vo. pp. 118. London, 1826. Longman and Co.

THAT the capillary system, or as Dr. Black quaintly terms it, "the debateable land," which connects the arteries with the veins, and which barely acknowledges the sovereignty of the heart, involves in its mysterious windings the proximate cause of inflammation we do not doubt, after the microscopical observations of Philip, Hastings, and Thompson, have so well established the fact. Our author, however, has presumed that further inquiry might, "at least serve to place the data already afforded in a more impressive light" to himself,—to "confirm the conclusions already made, or to lead to some tolerably certain physiological or pathological doctrines." With this view, he performed many experiments on the webs of frog's feet, after the manner of Thomson, the results of which have furnished the following corollaries:—

"1st. The greater part of the capillary blood vessels are distributed in a reticular manner, the innumerable meshes of which chiefly affect the quadrangular form; and this tissue is principally composed of veins, and is intersected and supplied by more linear capillary arteries.

"2dly. The more natural and undisturbed the circulation, and the stronger the animal, the greater is the velocity of the blood; except sometimes immediately after the application of a comparatively slight stimulus, which increases the circulation a little.

"3dly. The healthy and natural circulation in the capillaries depends directly on a tonic or *tensive* state of their delicate tubes; and mediately, on some power inherent in the part, a degree of which power seems to reside in the blood equally with the vessels, and its energy depends more on the integrity or vitality of the part or parts of this system, than upon any impulse derived from the heart.

"4thly. The effects of a stimulus are according to the length or intensity of the application; and as regards the vessels to which it is applied, they are proportioned to the vitality of the part, and to the pressure of the column of blood in the leading arteries.

"5thly. A strong stimulus suddenly applied, in a short time produces a contraction of all the fibres of the part, and a diminution of the diameters of the whole classes of vessels, but more particularly of the arteries, which is followed at a greater or less distance of time by a corresponding dilatation, according to the extent of which, the phenomena of inflammation will be more or less observable; and more so if the *vis a tergo* be powerful or stimulated.

"6thly. A gentle or lesser stimulus produces an evanescent contraction, followed by relaxation, increased redness, and a slight acceleration of blood in the *leading* capillary arteries and veins; owing, it seems, to the increased capacity of the distended reticular vessels, while the former are yet relatively contracted.

"7thly. The circulation in the capillary system is independant of the control of the heart, except so far as this organ affords a constant pressure and a ready supply of blood upon which the capillary vessels may act; and it is still more independent of the brain.

"8thly. There are three states in which the capillary system may be situated, the more perfect degrees of which are, 1st, an *atonic* or *collapsed* condition of the vessels, wherein no circulation takes place, nor can red blood be observable, but the return of both which can be more or less easily accomplished. 2ndly. The *tonic* or *tensive* state which is the natural one, and wherein the circulation is brisk, uniform, and capable of being slightly accelerated by heat and moderate stimuli. The 3d is the *turgid, distended*, or *congested* state, wherein the blood has a diminished velocity; but if dilatation has not reached a certain point, contraction and a quicker circulation can be restored. This last condition is a state of atony opposite to

the first,—both, however, depending on weakness or exhaustion in the part, but with a different contingency of the *vis a tergo*, or the pressure of blood from the larger arteries.

"9thly. The *vis a tergo* being a constant force, the velocity of the blood is inversely as the diameters of any set of vessels, compared with the capacities of those which immediately succeed them; thus congested capillary veins, and a quick circulation in the larger capillary arteries, are quite compatible.

"10thly. The condition of a part, in what is termed inflammation, is essentially seated in the capillary vessels, and primarily and chiefly in those of the veins.

"11thly. Inflammation and congestion affect a portion of the capillary tissue, more from a local or topical condition, than from any action communicated by the vessels leading to the part.

"Lastly. Congested and even stagnant blood may be brought again into circulation, while a part that has been deeply inflamed or turgescent, in a short time may be found pale, flaccid, and collapsed, in a dying animal."

Dr. Black briefly adverts to the researches of Haller, Hunter, Spallanzani, Thomson, and Wilson Philip, into the interesting subject of capillary circulation, which are well known to the reader, and proceeds to propound the nucleus, as will shortly be evident, of any new theory, either of inflammation or of fever, that he may have to offer. "It may be useful to inquire," he says, "what assistance our knowledge of what is called the *principle of vitality*, or of that power by which the capillaries act, independent (ly) of the heart or brain, can legitimately avail us in the more extended consideration of capillary phenomena." Here he thinks he may "let slip the pen," and speculate on the properties of this principle, which are palpable to the senses; that what has been allowed to Newton in his doctrine of gravity, as a principle of matter, may, with "*nearly* equal fairness, be granted to the physiologist in his reasonings on the principle of life." Let it be granted, then,

"That a muscle contracts when irritated in the living, and in the recently dead animal; and that the capillary vessels, under certain irritations, also enlarge and diminish in the diameters, with corresponding alterations in the velocity, and in the volume of blood circulating through them, without either the heart, brain, or spinal marrow influencing or sympathising in the local affection, are facts which experiment has fully confirmed."

The question is, what are the nature, properties, and extent of that power, whether it be the result of, or a subtle principle superadded to, organisation, by which these effects of irritation are produced? We need not refer to the observations or experiments of Haller, Hunter, Bichat, Le Gallois, Philip, or any other physiologists, to convince us of the existence of some principle of life, whatever it may be, nor that it pervades every part of the system. But we do positively aver *in limine*, though it should marr the beautiful theory which the Doctor has reared, that there is neither truth nor reason in his saying, that

"The parts endued with this principle, (muscular power or contractility,) perform their functions, *or answer to natural or artificial impressions, independent of the nervous power*, or of that influence which emanates from the brain and spinal marrow, *though they are subject to its control*"!

How parts, which are "independent of the nervous power," can be "subject to its control," we certainly do not profess to understand; but we grant the doctor's postulate for the present, that the reader may see the evolution of his theory, and then proceed to demolish the whole; for by taking away this prop, the entire fabric will fall to the ground. The most obvious property, then,

- - - - - - "by which this principle is manifested, is that of contractility of all fibres having a muscular structure or character; a *lesser* visible degree of the same power, is *a state of tensiveness* affecting the same structures. These phenomena of contraction and tensiveness, obtain not only in the muscular textures, commonly so called, but also in the more attenuated structures and membranes, such as the capillary blood vessels; and there is *every reason from limited* proof, to believe that some degree of them affects the red globules of the blood itself, under the influence of appropriate stimuli."

How far this principle can "ever absolutely *accumulate* to an excess beyond what is necessary to the perfect integrity of all the functions and parts, and to the prejudice of health," the author will not venture to determine; "but that its relative deficiency or lesion in any part, organ, or in the body in general, is essentially connected with derangement or disease," may not, he thinks, be doubted. Having premised that the contractile vigour of a muscle, as well as secretion, is made to depend on this muscular principle, and "not on the nerves leading to the part," which are "only the *media* through which the respective stimuli are transmitted." The application of the theory to the first stage of fever is shortly this, "that it is not the sensorial or nervous energy, but the materia vitæ, or muscular life of the system which primarily suffers." But perhaps we had better lay before the reader the theorems of the author's argument.

"1st. A fever essentially and primarily consists in a negative state of the muscular power, as compared with the physical organism or substratum of the body; and it appears, that it is sufficient to constitute the febrile act, for the heart and blood-vessels to be alone involved in this *relative* condition.

"2dly. This negative state may be produced either by exhaustion, as from heat, violent exercise, or stimuli; or it may be the direct or instanstaneous result of an injury, or of the application of debilitating powers, as of some poisons and contagions.

"3dly. A stage of atony, torpor, or collapse, however short, and sometimes obscure, but at least to embrace the heart and blood-vessels, is the initial condition of every symptomatic and inflammatory fever, as well as those of the lowest and most contagious type.

"4thly. This negative state of the muscular power is, in the first instance, either quickly followed by death, or, sooner or later, by increased frequency or quickness of pulse, arising from the stimulus and pressure of the increased volume of blood on the heart, now relatively reduced in its vitality. This frequency of the heart's action is also promoted by accumulating heat, the consequence of repressed secretion and perspiration, and by the sensation of pain or irritation being transmitted through the nerves to the heart or sensorium, or to both, thus exhausting the muscular power, and increasing the mobility of the moving fibre.

"5thly. All fevers, commonly so called, are the constituted sequences of a prior condition; they are strictly physiological reactions.

"6thly. The reaction in any case will be as the previous strength and plethora of the constitution, joined to the quickness and to the degree to which the materia vitæ has been affected, provided it has not suffered to an immediate irretrievable extent; and the mildness of a formed fever will be proportioned to the small extent to which the muscular power has been injured; while the lowness and putridity of the type will again depend on the depraved or reduced state of the body previous to the disease, added to the greater or less universal lesion of the muscular power.

"7thly. A fever will recur and continue at the least until the *inertia* and bulk of the solids and fluids of the body be brought on a par with the powers of the muscular life, either by the reduction of the former, or by the increase of the latter; which equilibrium not being obtained, dissolution must follow.

"Lastly, the exact causes of idiopathic fevers assuming the different periodical types, or being continued, are obscure; as we are, in one regard, ignorant of the nature of the exciting causes, beyond perceiving that they have generically a debilitating effect on the muscular principle.

From what has been stated of the author's hypothesis, it will be seen that the whole solid and fluid parts of the system are deprived of their *materia vitæ*, or are, at the ingress of fever, possessed of a less proportion of their living and motive powers than they had during health, so that while the materia corporis, or "substratum" of the body remains the same, a part of the materia vitæ has flown out of the window. The "substratum," therefore, must be brought to "an equilibrium with its vital associate," which may be done in two ways, 1st, by venesection, which reduces the "substratum," or, 2dly, "by timely and gentle excitation," which adds to the "vital associate;" add to one, or subtract from the other, in short, bleed or give a dram, and the business is done! Bellini, we believe, in his book *De Missione Sanguinis*, first started the theory of obstructed capil-

laries, and the remainder of the author's doctrine does not differ materially from the dogmas of Browne. Of the parts endued with his *materia vitæ*, whatever that may be, "performing their functions," or answering to "natural or artificial impressions, independent (independently) of the nervous power," we shall say nothing, since the excellent paper of Mr. Charles Bell "on the nervous circle which connects the voluntary muscles with the brain," (Phil. Trans.) must have sorely dumbfounded this unfortunate author. Because the circulation in the web of a frog's foot was continued "for twenty minutes after the brain had been *crushed*," there was no necessity for building on that circumstance a theory of fever!

CHEMISTRY.

New method of purifying Crystals.

Every practical chemist knows how difficult it often is, particularly in the analysis of organic substances, to clear away from crystalline products the mother water, and other heterogeneous matters which collect in their interstices. When the crystals are very fine, and still more when they are soluble in the ordinary menstrua, it is sometimes impossible to clear them, although perfectly pure, by any other method than repeated crystallisation and digestion with animal charcoal, both of which processes are troublesome, and occasion considerable loss. The process depends on the transmission of a current of air through the crystals. The simplest apparatus for effecting this purpose, consists of a double-mouthed bottle, with a funnel in one mouth and a bent tube in the other, the lower opening of the funnel being obstructed by a ball of cotton wool, and the crystals being deposited upon the cotton.

On sucking the air through the crystals by a bent tube they are cleaned in a few seconds, and, if necessary, the operation may be repeated after previously introducing a little water into the funnel. A convenient way of constructing the apparatus, so as to work of itself, is to make the second tube reach the bottom of the bottle with one limb, and with the other a vessel of water situated in a lower level. The whole bottle and tube being filled with water, the funnel is to be introduced, and the water then allowed to run off by the syphon. On a large scale, a more suitable apparatus will be a tube from a steam boiler, by which the bottle may be filled with steam from time to time.

The steam communication being shut off, and the steam in the bottle condensed, the stream of air will immediately carry through with it the whole of the mother water from the most silky crystals.

ZOOLOGY.

On Female Birds assuming the Male Plumage.

The interesting fact of female birds assuming the plumage of the male, was in modern times first attended to by John Hunter, who, in a memoir on this subject, published in the Philosophical Transactions, described a hen pheasant and peahen which had in old age assumed the male plumage. Geoffroy St. Hilaire says, that of the many peahens in the *Jardin des Plantes*, no instance occurred of the peahen assuming the male plumage; a fact which shows that such a change is rarely met with among these birds. In the Museum of the University of Edinburgh, there is a fine specimen of the peahen with the male plumage, presented to the Museum by the Duchess of Buccleugh, and it was said in the note which accompanied it, that it was effected in a few years. The dorsal feathers are still more or less mottled with grey, and the green tint is lighter than in the feathers of the male. The rump feathers are elongated, some of them to the length of eighteen inches; but the ocellar spots are neither so large nor so varied as in the male. The ordinary tubercles on the tarsi of the female, have been developed into thick, regular, conical spurs, about half the length of the male. In short, the change is so much advanced, that after another month it would probably have been complete.

In the British Museum there are several specimens of female pheasants with the male plumage; two of these are remarkably fine, and were shot in Kent, near Cranbrook, by Thomas L. Hodges, Esq., and presented by him to the Museum. In the Museum of Edinburgh, there is a similar specimen by Dr. Hope. The only differences which the plumage of this bird exhibits, when contrasted with the male, are the following: first, the tail feathers are shorter than those of the adult male, although considerably longer than those of an ordinary female; secondly, the lustre of the colours in general is not quite so vivid as in the male, especially on the back of the wings

There is no appearance of spurs. Sometimes the same sort of apparent change of sex is observed among domestic poultry. Mr. Neile, at Canonmills, had a black hen, of what is called the French breed, which, in her twelfth year, ceased to lay eggs, and gradually assumed somewhat the appearance, and, to a considerable degree, the manners, of a cock. The principal change of plumage consisted in the tuft on the head becoming thinner, and showing some upright stray feathers, and in a single elongated feather projecting from the tail. The spurs were larger than usual in hens, but these had probably been increasing for some years. The change of manner of the bird was quite remarkable; she strutted about with a firm pace and raised tail. She formed a party among the fowls, which she led separate from the cock; and she roosted apart from him. She became very voracious, and where food was set down (losing all resemblance in this instance to the generous male,) she beat off the other hens. She soon became very fat, and died in a few months, seemingly from overfatness. Her cry was altered, but it had not much resemblance to the crying of a cock.

In a valuable paper by Dr. Butter, of Plymouth, in the third volume of the Memoirs of the Wernerian Society, there are many interesting facts on this subject, from which we extract the following table.

Table of such Birds as have in advanced life assumed the plumage of the male, with the names of the authors who have noticed the fact.

Order 4. Gallinæ—Domestic Fowls.

Gen. 1. Pavo, peahen	Hunter.
2. Meleagris, turkey	Bechstein.
3. Phasianus colchicus, common pheasant ..	Hunter.
P. Pictus, golden pheasant	Blumenbach.
P. Gallus, domest. fowl	Tucker, Butter.
4. Tetrao perdrix, partridge	Montagu.
5. Columba, pigeon	Tiedemann.

Order 5. Grallæ—Waders.

2. Family pressirostres, G. 1. Otis busand	Tiedemann.
3. Cultirostres, 3. Tribe, G. 4, pelican of America.....	Catesby.

Order 6. Palmipeda—Web-footed.

2. Family lamellirostres, soft skin on the beak.	
1. Anus, duck, common and wild	Tiedemann.

LONDON MEDICAL SOCIETY.

At the Meeting of the Society on Monday evening, the 11th inst., the discussion on the accuracy of Dr. Barry's theory, "that the atmospheric pressure acting upon the vacuum formed in the chest during inspiration, is the chief agent in the return of the venous blood," was renewed. In consequence of its having been announced in this Journal, that Dr. Barry was to be invited, the supposition became very general that the Doctor would be present, and the result was, that the room was exceedingly thronged, much more so than at any former meeting during the season. Mr. Ellerby mentioned to the Society some experiments which he had made on the subject, which appeared to militate against some of the conclusions of Dr. Barry. We know not how it happens, but it is pretty evident that a great stir is making in certain quarters to upset the Doctor's favourite doctrine; and unless he makes a vigorous stand against his opponents, and meets their objections *seriatim*, he is likely to suffer from their numbers, if not from their skill. As it is a dispute which can only be decided by experiments, we hope that these experiments will be undertaken with great care; and strict impartiality shall be observed by us in recording them.

Dr. Clutterbuck informed the Meeting of the subject of the evening's discussion, and took that opportunity of briefly explaining the opinions of Dr. Barry respecting the circulation. He said, that many years ago it had been advocated by Dr. Carson, of Liverpool, that the vacuum formed on the right side of the heart, on the contraction of the right auricle, exercised a considerable influence on the return of the venous blood; and that, in point of fact, the heart might be regarded as a sucking, and as a forcing pump, using the word sucking in its popular acceptation. It had, however, been shown, that the action of the arteries, the pressure of the muscles on the veins, and other causes, materially assisted in carrying on the circulation. He by no means wished to be understood as doubting the accuracy of Dr.

Barry's experiments; but he thought that few, if any, of the conclusions which had been drawn from them, could be legitimately supported. Mr. Ellerby had communicated to the Society some experiments, the results of which appeared to be at variance with the results of some of Dr. Barry's experiments conducted in a similar manner. He believed that some further experiments on the subject would be mentioned by that Gentleman during the meeting. The noise occasioned by an additional arrival of visitors, made the President's voice almost inaudible; but we understood him to say, that there were some circumstances of every day occurrence, which seemed to go against the theory of Dr. Barry; he might refer to the most simple, the flowing of blood from an opened vein. The current indicated no such rising and falling as might be expected, if the venous blood moved toward the heart during inspiration only, but it continued to flow in an equable stream. The President then alluded to the difficulty of accounting for the hepatic circulation on the principle of suction, and concluded his remarks by briefly adverting to the experiments on absorption made by Mr. Ellerby with the ferule, and by expressing his opinion as favourable to the conclusion arrived at by Mr. Ellerby, that pressure on the parts surrounding the poison, and not the vacuum formed over the poison, was the main cause of its not becoming absorbed.

Mr. Ellerby said that he had repeated his former, rather than made any new experiments. He by no means meant to say that the experiments of Dr. Barry were misrepresented, but that the conclusions drawn from them could not be supported. He had poisoned a large dog, by giving it some prussic acid, and having laid open the chest, he found the heart pulsating, although the respiration had ceased; he laid open the pericardium, and having put his finger on the trunk of the vena cava, about one inch from the heart, he made pressure on it, and prevented the further return of the blood; he then pressed the blood onwards, lying in the vessel towards the heart, and then compressed the vessel close to the auricle; on removing the lower finger, the blood immediately ascended. It would be difficult to attribute that ascent to the influence of the vacuum formed in the chest, since, if such vacuum had existed, its influence would have been rendered nugatory by the pressure on the venous trunk. He had also caused a vein to be opened in his own arm, the blood flowed regularly through the opening as usual; he then made some deep inspirations, but the blood continued to flow as before, not the least variation could be detected in the size or in the jet. He then held his breath for some seconds, that is to say, did not expire until the uneasiness in the chest became urgent, but no variation was observed in the flow of blood from the vein.

Mr. Pereira made some judicious observations on some experiments which he had assisted in performing at the General Dispensary. He referred more particularly to the more frequent contractions of the auricle to the ventricle, after the chest had been laid open, and the lungs collapsed; and to the hepatic and placental circulations, as so many arguments against Dr. Barry's theory.

Mr. Wigon apologised for the non-attendance of Dr. Barry, by saying, that he had promised to attend the Meeting of the Hunterian Society on Wednesday evening, to give a systematic account of his opinions on this subject; he was unwilling to anticipate that occasion by entering into a partial explanation at any other Society previously.

Mr. Langstaff observed, that if pathological anatomy had any thing to do with the matter, he thought that there would be some difficulty in accounting for the production of the required vacuum, where the lungs were hepatised, or in contact with the costal pleura; or where the heart, as was not rare, was in contact with the pericardium.

Mr. Wigon attempted a reply. It is a singular circumstance, that Mr. Wigon was the only advocate of Dr. Barry's theory in the room.

A discussion then arose on the proofs afforded by comparative anatomy against the theory. Fish, which have no lungs and only a single heart; certain reptiles also, and insects, have no lungs; yet in these the circulation is carried on in the veins. Cuvier and Dumeril raised the same objections in their report to the Institute on Dr. Barry's paper.

Mr. Serle showed the Society a very ingenious contrivance, which he thought would satisfactorily prove that during ordinary respiration, no movement of the blood takes place in the veins, in consequence of any vacuum being formed in the chest; that is to say, when the respirations are about twenty in a minute; but if the respirations were increased to forty in a minute, it would be seen that a considerable vacuum was formed, and that under such circumstances only, the blood returned through the veins with greater velocity to supply the vacuum. The apparatus was simple and ingenious; an ordinary shaped bellows without a valve in the bottom, with a pane of glass in the cover to allow of seeing what was going on in the interior, with a nozzle of the usual size, was produced. From the sides of the bellows three pipes of different sizes, bent at right angles, were passed into three glasses containing water. Mr. Serle stated,

that he considered the bellows to represent the capacity of an ordinary chest; the nozzle of the bellows to represent the same relation of size of cylinder to the bellows, that the trachea does to the lungs. If by raising the cover of the bellows, the vacuum were formed more rapidly than the calibre of the nozzle could supply air, which happened when forty such artificial respirations were formed in a minute, the atmosphere pressed the water in the glasses immediately into the chamber of the bellows; and by parity of reasoning, admitting the similarity between the respiratory apparatus and the bellows, he thought that the inference could not be refused, that the blood in the veins would then, and only then, be forced in to supply the vacuum, as being the most readily yielding matter on which the atmosphere could act.

The discussion was of an interesting character, and will probably be resumed at the next meeting.

Dr. Barry attended the meeting of the Society on Monday last. He said, he appeared there as a volunteer, and did so for two reasons: to show the Society that he was not reluctant to attend; and secondly, to convince the gentlemen present that he was always ready to break a lance with his opponents in the controversial arena.

Dr. Barry was then solicited by the Chairman, Dr. Haslam, to favour the Society with a brief *exposé* of his opinions respecting the circulation, which he did in a very clear and satisfactory manner; but as the arguments which he urged were very similar to those which he employed at the Hunterian Society, it will be unnecessary to repeat them here. Dr. Barry took considerable trouble to prove the influence of atmospheric pressure on the human body; he adduced the case of Humboldt, who ascended the lofty Chimbaraço, until the atmosphere became so rare that blood flowed from his eyes, ears, and gums. Here we may be allowed to observe, that the Doctor occupied considerable time in seeking to establish what no person denied. He proceeded to say, that Nature, ever fertile in her productions, had applied one of her laws, and that a very powerful one, to the assistance of organic matter; the pressure of the atmosphere was made materially to assist in the performance of the most essential functions of life. It was fair to presume, that such an uncommon power would not be left unemployed by nature, and he hoped to be able to show that it was employed. The Doctor then approached the more immediate subject of the discussion; he explained what he meant by a relative vacuum; it was of no consequence what the difference in the amount of pressure was, between the internal and external parts of the body; if the pressure in the interior were 5000, and the pressure externally 5001, it would be sufficient for his purpose, because it would prove that the resistance within was less than the force without. He then enumerated some experiments to show the amount of this difference of pressure in the interior. He connected a barometrical tube, containing the usual column of mercury, by means of a flexible cylinder, with the thoracic cavity of a dog weighing only six pounds, by perforating the thorax between the ribs. At each inspiration it was observed that the mercury sunk *two inches and a half*, showing that the resistance had been diminished equal to about one-twelfth of our atmosphere. The trachea being severed, and a stop-cock attached firmly to it, the stop-cock was closed, and at each inspiration the mercurial column descended *five inches*; proving that a vacuum had been formed, or, in other words, that internal pressure had been diminished equal to about *one-sixth* of an atmosphere. He had tried a similar experiment on a pigeon, with a similar result. These experiments tended to show, that the vacuum formed in the chest was very considerable; not sufficient, it was true, to lift thirty-two feet of water, like a city pump, but sufficient to show that the vacuum was something more than the mere moonshine, which some had represented. (A laugh.) He was led to conclude, from observing the atmosphere to act on the columns of mercury in these experiments, and on columns of water in others, and that only during inspiration—he was led to conclude, that the fluids of the body were subjected to the same general laws; that these fluids were forced toward the part where the resistance was least, namely, toward the thorax, and that this centripetal force acted with effect only during the expansion of the thorax, that is, during *inspiration*.

Dr. Barry complimented Mr. Ellerby, Dr. Davies, Mr. Serle, and the other gentlemen, for the manly manner in which they had conducted their experiments, and concluded by observing, that however great the differences of opinion might be between them at present, that he was satisfied they would eventually be removed.

We regret that our limits do not allow us to follow the series of Doctor Barry's reasoning so closely as we could wish, but we hope we have secured the principal points of his argument.

Mr. Ellerby, Mr. Serle, and a few others, made some observations.

The meeting of members and visitors was very large, and the thanks of the company were *una voce* presented to Dr. Barry for his attendance, and for the information he had afforded.

HUNTERIAN SOCIETY.

A CONSIDERABLE anxiety has of late prevailed among the different medical men of the metropolis to sift the opinions which have been recently advanced by Dr. Barry, respecting the influence of the pressure of the atmosphere on the circulation and absorption. It is probable that Dr. Barry has attributed a greater degree of importance to this agency than it will ultimately be found to possess. However this may be, we shall endeavour to give the progress of the discussion, and ultimately a resumé of what has been furnished *pro* and *con*, with our opinions on the subject. We abstain from saying more at present.

Dr. Barry was present at the last meeting of this Society at the request of the President, and in consequence, the attendance of the members was unusually large.

SIR WILLIAM BLIZARD was in the Chair, who briefly informed the meeting of the principal object of their being brought together. The Society, anxious to acquire and to extend information, was desirous of hearing from the learned Doctor who had that evening honoured the Society with his presence, a development of his ingenious and very philosophical opinions respecting the influence of atmospheric pressure on the circulation, and on absorption; and then, having loaded the Doctor with a profusion of compliments, sat down!

Dr. BARRY then addressed the Society, by first alluding to the *extreme honour* conferred on him by such a *distinguished and scientific body*, in being permitted to occupy a portion of their *valuable time*, and by assuring them that however *high he esteemed* such an *especial* mark of their *infinite condescension*, by inviting him there that evening, he hoped that it would *not* induce him to think *less humbly of himself*. It had been said that "he had felt hurt" at receiving such an invitation; but on his honour he could assure the President, that he considered the *very handsome* and *very polite note* of the secretary containing such invitation, as *the greatest mark of distinction* which had been paid him since his return to this country; and he should *ever preserve and esteem it* as one of his *most valuable* documents.

This sort of language may do very well in France, but it sounds mawkish in the ears of an Englishman; and as we heartily wish the Doctor well, we recommend him to leave off this superfluity of Gallic nothingness.

Dr. BARRY then proceeded to give an outline of his opinions respecting the circulation, but as we cannot insert his observations at length, we shall lay hold of the main points of his theory. Dr. Barry stated that the main argument on which all his reasoning was founded, the foundation stone on which his theory of the circulation was built, was that during *inspiration*, there is a *tendency to form a vacuum* in the chest; that this vacuum might be correctly termed *a relative vacuum*, and he would explain what he meant by a relative vacuum in the following manner: suppose an elastic bottle, made, for example, of Indian rubber, to contain one cubic inch of air; extend that bottle to twice its original length, and its capacity would be increased so as to be capable of containing two cubic inches of air; then there would be a relative vacuum formed; for although the bottle continued to be full of air, it was not equal to the resistance of the external air, in the proportion of two atmospheres to one.

Again, take a cubic inch of solid matter capable of condensation, and reduce it by pressure to half its size, there would be in that case also a relative vacuum formed, in as much as half of the original space would remain unoccupied. (Of course this is purely an imaginary case.) He then endeavoured to transfer this sort of physical reasoning to the condition of organic bodies in the performance of the functions of respiration and circulation. He contended that whether by the expansion of the thoracic parietes on the one hand, or by the contraction of the heart on the other, or by both, a relative vacuum was formed in the chest subjected to the action of the same laws of physics, as if formed in unorganised or inert matter.

In the course of the evening Dr. DAVIES asked Dr. Barry whether he attributed the return of the venous blood *solely* to this vacuum? Dr. Barry replied, that he considered the *vis a tergo* the *first* power; *atmospheric pressure* acting on the vacuum in the thoracic veins, *the second*; and the power of *gravitation*, the *third*.

Towards the close of the meeting, Mr. SERLE showed an experiment which he had made on a rabbit, to prove that the vacuum formed in the sinus venosus, if any be formed there, could not be of any consequence. He removed the sternum and portions of the ribs of a full grown rabbit, and then laid open the pericardium to such an extent, that the finger might be introduced.

Mr. ELLERBY and Dr. DAVIES called the attention of the Society to the result of some experiments which they had made on absorption with the ferule, instead of with a vacuum; they contended that they had shown that mere *pressure* was sufficient to prevent absorption, and that no vacuum was necessary for that purpose.

Dr. BARRY admitted that Mr. Ellerby had succeeded in showing this, but contended

that his cupping-glass acted in the same way; which is, indeed, the fact; but then the entire value of the Doctor's corollary disappears, because it was intended to establish the efficacy of a *vacuum*, and *not* the efficacy of *pressure*. Dr. Barry was induced to say, that from the report he had read of Mr. Ellerby's experiments in THE LANCET, he imagined that he had not read his work. Mr. Ellerby assured the Doctor that he had done so with much attention, but found nothing said therein on the effects of *pressure*. The Doctor apologised, and the meeting soon after separated.

MEDICAL JURISPRUDENCE.

To the Editor of THE LANCET.

SIR,—There has been no part of our science which has occupied so little the attention of its professors as medical jurisprudence. Whether this has arisen from the idea that their attention should be exclusively directed to the relative workings of the machine, when perfect and when impaired, rather than to the power by which it was first impelled, and afterwards kept in motion; and, perhaps, acting on the old maxim, *divide et impera*, they thought it ought to be left to the metaphysician and the lawyer; or whether they have considered its utility and importance not to be sufficient to warrant an attention to the subject, is uncertain; perhaps both motives have prevented medical jurisprudence from bearing its due proportion in systems of medical education. For certain it is, that it has been considered, until lately, unworthy attention, to adduce but one instance in the person of the illustrious John Hunter. That great man, possessing in an eminent degree a clear and comprehensive mind, though too often indefinite and obscure in expressing his ideas, showed, by his evidence in the case of Sir Theodosius Broughton, that he had turned his attention but slightly to forensic medicine; for it may be clearly detected, that the fault was not in the language, but in the ideas. But though the subject has lately been brought into notice by the works of Paris, Fonblanque, and Dr. Gordon Smith, yet it has been comparatively neglected, and it is but seldom, even now, that we read medical evidence that is worthy of the members of a yelept liberal profession,—men who, considering the intimate connexion and action and reaction of the body and immaterial principle on each other, ought to be well acquainted with the philosophy of the human mind. For the truth of this assertion, let any one refer to the evidence in Lord Portsmouth's case, the definitions of insanity are most absurd as Dr. Armstrong justly observes, in his Lecture on Mania; they might as well have said his complaint was a *thing-um-bob complaint*, or a *cloud coming over him*, or any thing else. It is but a short time since that my attention was attracted to the evidence of a surgeon by the name of Dewsnap, at a Coroner's Inquest, in a case of suicide, in which he attributed it to *temporary excitement of some kind or other*. Now, by analysing this, what are we to understand? *Excitement* is a term, it is well known, introduced into medical literature by Dr. Brown, the founder of the Brunonian system. By his definition of it, we understand that it is that property by the possession of which animals differ from themselves in their dead state, or from any other inanimate matter; hence, being the essential of life, it cannot be *temporary* or *changeable* in its *kind*. But since the time of Dr. Brown, the word has been used in a much more extended sense in medical language, and by it we now understand that state which is the effect of *stimulants*, and attended with increased sensibility and irritability of the nervous system, and increased action in the sanguiferous. This state, of course, is necessarily *temporary*, but differs not in *kind*. Now, allowing this to be the proper construction of the expression, the question naturally follows, Is this state ever the precursor of suicide? I apprehend very few medical men would hazard an affirmative. It may, however, be useful to discuss what is that state? I should answer, a state of *depression* caused either *mentally* by the *immediate* operation of the depressing agents, or *intermediately* by the exciting agents, causing *collapse* after excitement, or *bodily* by chronic inflammation, producing a change of structure in the brain, and the immaterial principle thus performing its functions through an impaired medium, the mind being the effect, is of course impaired.

I trust, by the insertion of these unconnected observations in your widely circulated Journal, that it may in some measure excite the attention of the profession for their own *credit* to medical jurisprudence, that they may no longer make use of "words full of sound signifying nothing," leaving out the more important consideration, that in giving evidence in a court of justice as in their ordinary employment, "the issues of life and death are often in their hands."

I am, Sir, yours,

ATTICUS.

NEW COLLEGE OF SURGEONS.

[We feel it a duty to insert this Letter, but we do not approve of many parts of the plan it proposes for a new College.]

To the Editor of The Lancet.

Sir—The object of my last communication was to recommend the establishment of a National College of Medicine and Surgery in the place of the present Royal and Chartered Companies, who, it is admitted, upon almost every side, have, by their meanness, selfishness, and imbecility, brought themselves and their profession into merited contempt. It now remains for me, in compliance with my promise, to present you with a brief outline of my plan for such an establishment.

Proposition 1. That a Hospital capable of containing at least 2000 patients, be erected at the expense of the nation.

2. That to this Hospital be appointed Professors, Physicians, Surgeons, &c.

3. That such professors, &c. be liberally paid by the nation for their services.

4. That such services be entirely confined and devoted to the National Establishment.

5. That such professors, &c. be elected by ballot from the present members of the incorporated bodies of physic, surgery, &c. by the members at large.

6. That such professors, &c. shall examine all candidates for admission into the profession.

7. That such examination shall be public.

8. That the present members of the incorporated bodies of physic, surgery, &c. be constituted members of the national college.

9. That pupils shall be admitted to the different lectures, and to the practice of the hospitals, upon the same conditions as are observed at the national hospitals at Paris.

10. That every one shall be permitted to study his profession in any part of the kingdom, or in any part of the world he may think proper, but that the test of his qualifications to practise, shall consist in his ability to pass a public examination to the satisfaction of the examiners of the National College, without any reference to the school in which he may have studied.

11. That from and after ——— no one shall be permitted to practise in any department of the profession, or in any part of the United Kingdom, without previously undergoing such examination.

These, Mr. Editor, are the leading propositions which I wish to submit to the consideration of my professional brethren, and to the nation at large. The object I have in view is of the highest professional, as well as the highest national importance; and most sincerely do I pray that it may meet with numerous able and powerful advocates.

In another paper, I will trouble you with a few comments upon the different propositions, taking them in the order in which they stand. In the mean time I remain,

Mr. Editor,
Yours, very respectfully,
A Constant Reader.

Just as I had concluded this paper, I received No. 166 of The Lancet, in which a Licentiate of the Royal College of Physicians has made it very apparent, that however necessary it may be for a man to be a gentleman in order to become a fellow of that College, it is not necessary to be a gentleman in order to be admitted a Licentiate; and, Mr. Editor, to assert that a man has "assumed a wilful falsehood" upon such slender premises, bespeaks a dearth and destitution of intellect, which I am charitable enough to believe would not have passed muster with the celebrated Examiners of the celebrated Dr. Last. It is evident, upon the Licentiate's own showing, that my statement is substantially correct. That a by-law of the Royal College does exist, by which *even* an M.D. of the Universities of Oxford or Cambridge, would be prevented from becoming a Fellow of that College, should it be known that he had ever acted as a general practitioner. Can any thing be more unjust, more ridiculous, more contemptible, than such a by-law! I may have been guilty of not accurately discriminating between the *pure* surgeon and the general practitioner; but, as far as my observation extends, general practitioners are, for the most part, better educated, better informed men—men of more enlightened views, than *pure* surgeons, consequently rendering the regulation of the College more invidious and objectionable. It would be curious, Mr. Editor, to trace the origin of the division of the faculty into pure physicians and pure surgeons: if I may be allowed to hazard a conjecture or two on the subject, I should suspect that it resulted from a combination of the idle with the weak; for example, a man of quick parts, but too idle to undergo the drudgery of the dissecting room, chose, for conscience sake, to betake himself to the practice of physic only, trusting for casual assistance to the dull, plodding, but mechanical and industrious anatomist; be this as it may, the annals of medicine will prove that the pure physician and the pure surgeon have contributed little to the amelioration of human

suffering, and that the great benefactors of mankind have been those who had capacity to study and to practise every branch of the profession. Look at the pure physicians and the pure surgeons of the present day; what are they but the veritable Simon Pures!

But to return: The information conveyed in my last communication, I received from a Fellow of the Royal College of Physicians; and of its accuracy, I could furnish many instances, but that I am limited for time.

To conclude: The insolence of the Licentiate in giving the lie under the shelter of an anonymous letter, and the personal remark with which he concluded it, and which he doubtlessly considers equally close and pithy, are in the very spirit of that monopoly, by which, alone, such as he can expect to raise themselves into the *smallest* degree of consequence or consideration.

DEMONSTRATIONS AT GUY'S.

To the Editor of The Lancet.

Sir,—Impressed with a full conviction of the many abuses in our Hospitals, which your highly valuable publication has already been the means of correcting; and knowing the great interest you have always taken in whatever affects the welfare of the students, and your readiness at all times to advocate their cause, I have been induced to trespass on your valuable time, and at the request of several of my fellow pupils, to solicit your kind interference with the great want, both of attendance and attention on the part of our *three* demonstrators in the dissecting room, which has been so severely felt by the class throughout the whole of the present session.

Not to occupy your time unnecessarily, it will suffice to inform you that as early as eight in the morning, the din of active preparations might be heard in the dissecting room, and by nine, there is scarcely a table unoccupied; but no demonstrators are there to assist us in our studies, and it is very rarely indeed, until the late hour of twelve, that we have any advantage from their presence. Our anatomical lecture begins at two, on our return from which, they have vanished; and hence, Sir, the short space of two hours, or two hours and a half, daily, at most, is all that is allowed, and doubtless considered by them, sufficient to furnish the necessary information to perhaps one hundred dissecting pupils; and even, Sir, during that short period, I regret to add, a few favoured individuals monopolize their chief attention, whilst the less fortunate, notwithstanding their repeated intreaties, are destined to pursue their work of toil with the assistance only of the London Dissector, or some other typographical guide.

Towards these gentlemen I entertain feelings of the utmost respect, and I beg, Sir, to assure you, that it was not until after the most mature consideration, and with very considerable reluctance, that I ventured to offer or trouble you with these remarks.

In the fullest hope, however, that they may be productive of the desired effect, should you consider them worthy a place in your esteemed publication,

I beg to subscribe myself

Your obliged Friend and Servant,

A Pupil of Guy's.

December 9, 1826.

HOSPITAL REPORTS.

HOSPITAL OF SURGERY,

Panton Square, St. James's.

SECOND CASE OF CAROTID ANEURISM, SUCCESSFULLY TREATED BY TYING THE ARTERY BEYOND THE ANEURISMAL TUMOUR.

Nearly a year ago, in our 122d Number, we recorded a case of carotid aneurism, which was *successfully* treated by tying the artery *beyond the tumour*. Our readers may recollect, that such an operation was first proposed by Desault, approved by Bichat, executed, *with a fatal result*, by Deschamps and Sir Astley Cooper, and treated with ridicule and contempt by that truly great anatomist, the late Mr. Allan Burns; consequently, Mr. Wardrop undertook the operation under the most discouraging circumstances, as the only means of saving his patient's life, and he has been gratified by complete success, for we learn that the old lady enjoys perfect health, and that not the smallest vestige of the aneurismal tumour remains; therefore Wardrop's name will be handed down to posterity, if not as the *originator*, yet as the probable *rescuer from oblivion, and certainly as the first successful practiser*, of an excellent operation—an operation which presents a cure for such aneurisms as, from their situation, were deemed beyond the reach of art, or which only admitted of palliative treatment; an operation, the safety and utility of which have been demonstrated by two consecutive cases.

Dec. 2. E. B., ætat. 57: always enjoyed good health till four years ago, when she was attacked with severe pains in the head, which terminated in a fit of apoplexy. By bleeding, the application of blisters, &c., however, she soon recovered. Two years afterwards she had a similar attack, and was ill for three weeks, but the same treatment being pursued she again was restored to perfect health. About six months since one of her companions accidentally observed a strong pulsation in her neck, which a surgeon discovered to be an aneurism of the right carotid artery, and proposed an operation, but the patient would not consent.

A very strongly pulsating tumour is visible on the right side of the neck, exactly under the sterno-cleido-mastoideus muscle, which appears much attenuated, and its two portions are somewhat parted. The pulsation extends from the clavicle about two inches upwards, and is much stronger at the superior than the inferior part of the tumour, which does not, seemingly, extend below the clavicle. It is fully two inches and a half in breadth; the external jugular vein is seen traversing the upper part of it, and is somewhat distended. The patient has had a hernia in the right groin for four years, and both her legs are œdematous, very tense, and painful. She complains of severe head-ache, and is unwilling to lie on her right side, because, when she does so, the pulsation of the tumour is increased, and is accompanied by much noise in the corresponding ear; sleeps little at night; appetite bad; thirst great; pulse natural in frequency, but with a slight thrill; bowels confined.

℞ *Hydr. submur.*
Pulv. antimon.
Ext. rhei, aa. gr. ii. M. ft. pil.,
to be taken at bed-time.

Dec. 3. Pulse fuller than yesterday; otherwise much the same; to lose ʒvj. of blood from the arm.

4. Symptoms much the same; urine scanty, and high coloured:

℞ *Pil. scillæ*, ʒj;
Pil. hydrarg, gr. xii;
Pulv. digitalis, gr. iv; M. ft. pil. xii.
one three times a day.

6. Urine much more copious; legs much reduced in size, and less painful; rests badly.

℞. *Pil. opii.* gr. ij. at night.

7. Slept well; is feverish to-day; tongue brown; thirst urgent; great sickness; to take 1 gr. opium.

8. Sickness gone; tongue still brown; bowels rather costive. To take some salts and senna.

9. Slept well; bowels regular; felt very giddy for some minutes this morning, and nearly fell from her seat.

10. Two grains of opium having been given in the morning, Mr. Wardrop, with the assistance of Mr. Lawrence, and in the presence of many practitioners, tied the carotid artery at its emergence from beneath the omo-hyoideus muscle, and above the tumour. As the patient's neck was fat, the incision made in the integuments was not less than three inches in length; the rest of the operation was chiefly accomplished by means of a silver knife, and not above a tablespoonful of blood was lost. The ligature, formed of a portion of silk-worm gut, as recommended by Mr. Fielding,* was readily conveyed round the artery by Bremner's needle, and after being tied, both ends were cut away. The external wound was secured by two stitches, and a strap of adhesive plaster.

The patient felt no immediate effect from the operation, except a slight faintness; she walked up stairs unsupported, and sat some time in a chair, before she would get into bed.

The effect on the pulse was very remarkable, that of the right wrist being full and strong, while that of the left was comparatively small and feeble.

In the evening, the patient felt comfortable, complaining only of a dryness of the fauces, attributed to the opium she had taken. She was now ordered to have five grains of calomel.

11. Rested much better last night than she has done for some time past; no headache; she feels more lively than before the operation; countenance improved; pulsation in the tumour much reduced, particularly in the tracheal portion. Pulsation of the opposite carotid increased in force. The adhesive plaster was taken away; wound adheres; no appearance of inflammation.

12. Pulse of the right arm continues much stronger than that of the left. Left carotid artery beats with increased force. Temporal artery of the right side can be felt, feebly pulsating. No perceptible change in the tumour since yesterday; whole wound appears to have completely adhered; no dressing required; no return of headache; health much improved; sat up and dressed herself, and is in excellent spirits. Tumour is easily diminished in bulk, by gentle pressure; but on its application, complained that the pain of the head returned. She experienced two slight attacks of faintness yesterday afternoon,

* See Trans. of Med. and Chir. Society of Edinburgh.

with pain in the bowels. Bowels confined; to take some salts and senna immediately.

13. This morning she feels perfectly easy, the medicine having operated well; is quite free from all those pains in the head which troubled her previously to the operation, and she can lie down in bed on the left side without that feeling of suffocation which was formerly the consequence of it. The pulsation is now confined to the external part of the tumour, and on pressing it the pains are reproduced (for the time) in the head, with the same violence as formerly. The two stitches were removed, perfect adhesion having taken place.

℞. *Pil. Colocynth Co.* ℈j;
Calomelanos, gr. iii. M. ft. pil. v.
one morning and evening.

Thursday, Dec. 21. The patient is entirely free from any uneasiness in the head; has an excellent appetite, and states, that in every respect she is "quite comfortable."

As this patient in all probability will soon leave the Hospital, those practitioners in town who are desirous of seeing her, had better embrace an early opportunity.

ST. THOMAS'S HOSPITAL.

CASE OF STRANGULATED SCROTAL HERNIA,

In which the symptoms were somewhat ambiguous. Gangrene of the Intestine. Operation, and fatal termination of the case.

William Poole, a chimney sweep, twenty years of age, was brought to the Hospital on Tuesday afternoon, Dec. 5, at five o'clock in the afternoon; he was seen by the dresser, reported to have strangulated scrotal hernia of the right side, and was ordered to the warm bath. Whilst in the bath, blood was taken from the arm to the amount of twelve ounces, and the taxis was employed for some time without success, when a messenger was dispatched to request the attendance of Mr. Tyrrell, the surgeon of the week.

It appeared from the patient's statement, (which, however, was by no means clear,) that he had laboured under hernia of the right side for several years, but had never worn a truss. The intestine, he said, often came down, but was in general easily returned by his assuming the recumbent position and using gentle pressure; on being more minutely questioned, however, he stated that there was always some degree of swelling at the part. In the present instance the intestine descended on Saturday evening (at first he mentioned Friday); he began to vomit on Sunday morning and had continued to do so at intervals up to the time of his admission.

Such was a brief history of the case, with respect to the local symptoms, at the time the patient was seen by Mr. Tyrrell; after removal from the warm bath, they were as follows:—On the right side of the scrotum was a swelling of the size of a double fist; it was tense, tender to the touch, and gave an indistinct sense of fluctuation; the integuments on its anterior surface were red and shining; the cuticle, on close examination, had a desquamatory appearance. The testicle could be distinguished at the back part of the tumour, and felt hardened and enlarged; tracing the swelling upwards along the chord, it was found to extend throughout the inguinal canal, and at this part to the internal ring was hard and incompressible. The swelling did not dilate in the least degree on coughing.

The abdomen was not very tense, but evinced some tenderness on pressure; the bowels had not been relieved for three days previously. There was occasional sickness, but no hiccup; the pulse was small and quickened, but had not that peculiar thready feel, said to be characteristic of peritoneal inflammation; the patient complained of a sense of constriction at the upper part of the abdomen; but there was no marked anxiety of countenance, and when he was directed to bend his body forward, he did so without evincing pain.

Having ascertained the foregoing particulars, still Mr. Tyrrell considered the case as dubious, and, therefore, previous to adopting any plan of treatment, procured the attendance of Mr. Callaway. This gentleman concurred in opinion with Mr. Tyrrell, that the case was ambiguous. The circumstances which he enumerated as constituting the ambiguity, were first, the peculiar shape of the tumour, it being unlike hernial swelling, which is usually seen to enlarge gradually from the external ring, whilst, in the present case, the tumour enlarged, as it were, abruptly. Next, the remarkable appearance of the scrotal integuments covering the tumour, which was very similar to the change that is produced in the skin when suppuration is going on beneath. Lastly, the constitutional symptoms were supposed not to be those characteristic of an intestine having been strangulated for three days; the absence of anxiety in the countenance, and of the vibrating pulse, were taken as evidences of the non-existence of strangulation. In this state of doubt as to the true nature of the swelling, it was at length determined upon, that a puncture should be carefully made at the lower part. This was

effected with a lancet, and a probe was introduced at the aperture; a few drops of blood only followed. It was agreed upon by Mr. Tyrrell and Mr. Callaway, that there was no urgency of symptoms, and it was therefore determined to try the effect of unloading the bowels by means of aperient enemas; and, in order to relieve the tenderness of the abdomen, leeches were directed to be applied, with the subsequent use of warm fomentations. Shortly afterwards Mr. Tyrrell left the Hospital, it being understood that Mr. Callaway would see the patient, and report on any material change that might occur.

About ten o'clock, Mr. Callaway visited the man, and upon attentive examination of the tumour, was induced to suppose that he could distinguish a gurgling noise on pressure, and rightly judging that this constituted an important feature in the diagnosis of the case, dispatched a messenger for Mr. Tyrrell, who arrived some time afterwards. The symptoms had not at this time become aggravated; the vomiting was occasional, and the pulse was of the same character; the appearance of the tumour was the same; the bowels had not been relieved. Mr. Tyrrell, however, after further deliberation with Mr. Callaway, came to the resolution of cutting down upon the tumour, and proposed this measure to the patient, who, for a great length of time, refused his consent, but at last yielded.

Operation.

An incision was made through the integuments, commencing from below the external ring, and extending a considerable way down the scrotum. The different layers of fascia were carefully divided, until at length the cremaster muscle was exposed—this covering was observed to be remarkably distinct, and it had contracted such an intimate adhesion with the hernial sac, that it was with much difficulty separated, so as to afford a clear space for the penetration of the sac. When the sac was punctured, a small quantity of bloody serum made its escape, and being fully laid open, the nature of the case was rendered apparent. There was a large portion of omentum, and lying under this, a fold or knuckle of intestine. Mr. Tyrrell with much difficulty passed his finger through the external ring, and immediately above this discovered a band of stricture, which he divided. At some distance higher up was a second band, which was also cut through, and then the finger passed on with tolerable facility to the internal abdominal ring, at which part it was again necessary to dilate. So that in point of fact there were three strictures—two in the inguinal canal, and one at the usual seat of stricture, namely, the internal ring.

With respect to the condition of the contents of the sac, which it now became necessary to examine previous to making any attempt to return them, it was found that a portion of the protruded omentum was in a state nearly amounting to gangrene; it was dark coloured, had an unnatural feel, and its vessels appeared to be gorged with black blood, which was stagnant. There were some adhesions, evidently of long standing, between the omentum and sac, so that the patient had had irreducible omental hernia. The protruded fold of intestine was of a dark brown or chocolate colour, and there were two distinct gangrenous spots on its surface, of considerable size. The intestine was adherent to the sac throughout the whole extent of the inguinal canal. An incision was made into the gut through one of the gangrenous spots, when a fluid escaped having a fœculent smell.

The patient was now put to bed, and a linseed-meal poultice was applied over the part. The following medicines were prescribed:

Calomel, 3 *grains*;
Opium, 1 *grain*; to be taken immediately.
Sulphate of Magnesia, 3 *drachms*;
Laudanum, 5 *drops*;
Peppermint-water, ½ *an ounce*; to be given every three hours.

Nov. 6. Three p. m. It is now rather more than twelve hours since the operation was performed, and the vomiting has not ceased to occur at invervals of a few minutes, until within the last half hour. We found him dosing, but he soon awoke up, and the vomiting recommenced, it was unattended with any exertion, a kind of passive vomiting, as if dependent on simple retroverted action of the stomach, without any convulsive action of the diaphragm. The fluid rejected was of a light brown colour, and had a fœculent odour. The pulse at this time is small, and not vibrating; he has, however, been bled from the arm, under the direction of Mr. Tyrrell, which bleeding, we understand from his dresser, materially altered the character of the pulse, it being previously jerking. The abdomen is not tense, nor very tender on pressure. The bowels have been relieved once; a colocynth enema has been administered since the operation.

He continues to take the saline aperient draught, with the addition of five drops of laudanum.

7. The vomiting has continued throughout the night, and but little sleep was obtained; the sister states that the sickness is more frequent this morning than last evening. The fluid ejected is of a yellowish colour. The process of separation between the dead and living parts has evidently com-

menced in the intestine; he passed an evacuation during the night, the principal part of which came by the wound. Poultices are continued. The pulse is more expanded than yesterday, the tongue is moist, and covered with a brown fur in the centre; the abdomen is not tender on pressure.

Eight ounces of blood were abstracted yesterday evening; a grain of calomel, with a like quantity of opium, were given; and an effervescing mixture, with a few drops of laudanum in each dose, was ordered to be given frequently. This morning Mr. Tyrrell directed half a grain of calomel, and half a grain of opium, to be given every second hour.

Evening. Seen by Mr. Tyrrell, who found the pulse somewhat sharp; a vein was opened in the arm, and four ounces of blood only abstracted. Ordered a small portion of brandy in water, and to omit the effervescing medicine.

8. The pulse soft and feeble; the abdomen is not tender; the vomiting continues, but is somewhat less frequent; the evacuations from the bowels are principally passed through the wound—a small portion only per anum.

The calomel and opium are continued. Mr. Tyrrell this morning directed some arrow root, with a small quantity of brandy, to be given.

9. The patient is evidently sinking; the vomiting has, in a great measure, ceased; but this is rather attributable to the low state of the vital powers than to any amendment. The pulse is feeble, and the countenance sunk; the calomel and opium were discontinued last night, and he was ordered an enema of strong beef tea. Brandy, with eggs, to be given frequently. This morning Mr. Tyrrell prescribed an enema composed of half a pint of beef-tea, with two ounces of port wine, and twenty drops of laudanum. This is to be given every four hours. A poultice of stale beer grounds to be applied over the wound.

The poor man, we regret to say, died in the course of the night.

Inspectio Cadaveris.

The body was inspected on Sunday noon by Mr. Tyrrell. It was found that the intestine, which had been strangulated, was a portion of the ileum, about ten inches from the colon. A complete knuckle or fold of the intestine had passed down, and the two patches of gangrene were situated at each extremity of the protruded portion of gut. The strictures had been freely and sufficiently divided, so that no impediment was offered to returning the intestine, provided its condition had justified such a measure. There was a general blush of inflammation over the peritoneum covering the intestines; but that lining the parietes did not show any traces of inflammation. There were red circular lines on the small intestines, but there was no effusion of serum into the cavity of the abdomen.

Mr. Tyrrell made some clinical remarks upon this case to the pupils on Monday last, and exhibited the morbid parts. In commenting on the case, he first took a retrospect of the local and of the constitutional symptoms which usually characterise strangulated hernia, and then contrasted these symptoms with those observed in the case under consideration, and thus endeavoured to show in what respects they materially differed, and hence rendered the case anomalous. As we have, in our report of the case, entered into a full detail, it is unnecessary here to recapitulate.

[This case, in many points of view, is highly instructive.]

OPERATION.

The only operation performed at this Hospital of late, in addition to the above, is amputation of the thigh by Mr. Tyrrell on Friday last. The patient was a young man, and had laboured under scrofulous disease of the knee-joint upwards of five years.—The disease had proceeded to such a destruction of parts, that the tibia was dislocated backwards; the patient's general health was suffering greatly.

TO CORRESPONDENTS.

An "Eating and Drinking Club," as they term it, has been established, and consists of "The Hole and Corner" Surgeons—the "Surgical Pioneers"—"The Ninnyhammers"—"The Exploratory"—"The Hagglers"—"The Lithotomists"—"The Hospital Maggots"—and those "straight-forward" Anatomical Lecturers who pursue "serpentine courses;"—truly, a most brilliant assemblage. General Practitioners are to be excluded. Will they incur the disgrace of seeking admission?

The communication of *Medico-Chirurgus* shall be inserted, but we are precluded from saying on what day, in consequence of its great length; it shall, however, appear as early as possible.

The Pupils at the Veterinary College complain, and with great justice, of the want of a MANUAL OF ANATOMY to be used in the Dissecting Room.

J. P. C. informs us that a VETERINARY HOSPITAL is about to be established in Edinburgh,—we hope it will be governed in a more creditable manner than that at St. Pancras.

We cannot speak with any degree of certainty of the reprinting of the Volumes mentioned by a CONSTANT READER. Dr. THOMSON's Lectures on Inflammation are out of print.

Mr. E. H. MOORE considers that an *address* to the mechanics, on the utility of anatomy to medical students, would be sufficient to remove their prejudice against dissections —we disagree with him, they must *see* the subject under the knife before they will be convinced.

We thank Sir FRETFUL for his hint—the *Exhibition* is ridiculous enough.

"A LICENTIATE" should have recollected that Dr. ELLIOTSON has made no *anonymous* attacks. We cannot insert his letter unless authenticated.

We shall not descend to notice the stupid vapourings of L. C. SAYLES, the itinerant tooth drawer, of the Cow Gate, at SHEFFIELD.

Dr. FORSTER of Hartfield has signified his willingness to give us MS. notes of his Lectures on the Anatomy and Physiology of the Brain, comprehending strictures on the popular hypotheses of the mind of the present day—we shall perhaps avail ourselves of the Doctor's kind offer at some future period.

A STUDENT OF GUY'S HOSPITAL, wishes to learn why the pupils are not permitted to inspect the preparations in the Museum. This exclusion is in our opinion an act of gross injustice.

We have received "*The Circular*" sent by *Octavius Prichard*, M. D. to the inhabitants of Chichester,—we cannot take any further notice of it, as we never meddle with the proceedings of private practitioners, whether surgeons, physicians, or apothecaries.

The case of —— M. D. from L*** is not calculated for publication.

The communications of SENEX—R. L.—A RADICAL REFORMER—AN ENEMY TO INJUSTICE—A BOROUGH STUDENT—G. S. —SINCERUS—CHIRURGUS IMPURUS—A CONSTANT READER—E. H. HODSON—are under consideration.

The case referred to by E. T., shall not receive a further notice until we hear from him.

LIBER HOMO complains of the irregular attendance of the surgeons at St. George's Hospital. If Messrs BRODIE and Co. cannot be regular in their visits, they ought, in justice, to return the students their entrance money.

It appears that the disgraceful conduct of the bullying box-carriers of St. Bartholomew's Hospital, is still tolerated by the governing officers of that establishment.—The students should apply to the Grand Committee.

We will insert the letter of E. J.

The case of "Polypus of the Heart," by Mr. SIMPSON, shall appear at an early period.

We will either insert Mr. G's letter or offer a few remarks on the subject of it.

WORKS RECEIVED FOR REVIEW.

Observations on the Causes, Symptoms, and Treatment of the *Derangement of the Mind*. Founded on an extensive Moral and Medical Practice, in the Treatment of Lunatics. By Paul Slade Knight, M. D., formerly a principal surgeon in the Royal Navy, and many years surgeon of the Lunatic Asylum for the County of Lancaster, &c. London, Longman and Co. Robinson and Bent, Manchester. 8vo. pp. 167.

An Elementary Description of the Anatomy and Physiology of the Brain, Viscera of the Thorax, &c., with corresponding questions, designed for the use of Gentlemen preparing for examination at Apothecaries' Hall, &c. By W. Simpson, M. R. C. S. and Licentiate of the Society of Apothecaries. London, Anderson, (2d Edit.) 18mo. pp. 66.